Nasals on My Mind

The Phonetic and the Cognitive Approach to the Phonology of Nasality

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Für Mich.[1]

"Was soll nicht alles Meine Sache sein! Vor allem die gute Sache, dann die Sache Gottes, die Sache der Menschheit, der Wahrheit, der Freiheit, der Humanität, der Gerechtigkeit; ferner die Sache Meines Volkes, Meines Fürsten, Meines Vaterlandes; endlich gar die Sache des Geistes und tausend andere Sachen. Nur *Meine* Sache soll niemals Meine Sache sein. 'Pfui über den Egoisten, der nur an sich denkt!' " [Max Stirner][2]

"Das Göttliche ist Gottes Sache, das Menschliche Sache 'des Menschen'. Meine Sache ist weder das Göttliche noch das Menschliche, ist nicht das Wahre, Gute, Rechte, Freie usw., sondern allein das *Meinige*, und sie ist keine allgemeine, sondern ist—*einzig*, wie Ich einzig bin.
Mir geht nichts über Mich!" [Max Stirner][3]

[1]Translation (mine): To Myself.

[2]*Cf.* Stirner [463]. Translation (mine, adapted from Byington's translation from 1907 which is available on the internet [462, p. 4]): What is not supposed to be My cause [concern]! First and foremost, the good cause, then God's cause, (hu)mankind's cause, truth's, freedom's, humanity's [humaneness's], justice's cause; further, My people's, My prince's, My fatherland's cause; at last the cause of mind [intellect/spirit] and a thousand other causes. Only *My* cause is never to be My cause. 'Boo, shame on the egoist who thinks only of himself!'

[3]ibid., p. 5. Translation (mine, based on [462, p. 6]): The divine is the Divine's [God's] cause; the human, 'man's'. My cause is neither the divine nor the human, is not the true, good, just, free, and so forth, but solely Mine, and it is not a general one, but is—*one and only one* [unique] as I am one and only one [unique]. To Me nothing is more than Myself!

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Introduction

This thesis compares two approaches to the phonology of nasality and consists therefore of two main parts: the phonetic approach, which is discussed in part 1, and the cognitive approach (part 2). This is to say that this thesis investigates how the Language Acquisition Device employs nasality to define vocalic or consonantal systems of contrast, on the one hand, and phonotactic constraints and phonological processes, on the other. Ultimately, the phonetic approach is rejected, while the cognitive view is argued to be the more empirical one.

Part 1, which deals with the phonetic approach, has three chapters. In chapter 1, I show after a brief introduction to Popper's evolutionary view of research and empiricism, that the assumption that the phonologial behaviour of nasality or any other phonetically defined notion is phonetically motivated or grounded (the 'Phonetic Hypothesis', 'PH') is flawed. I make it clear that there is neither an articulatory nor an acoustic definition of nasality nor a well-defined phonetic framework on the basis of which one could predict the phonological behaviour of nasality. Concepts like 'velum lowering', 'nasal flow' or references to 'Formant 1' are not useful in a phonological analysis of acoustic data. Furthermore, I claim that the PH is always set up in an unfalsifiable or nearly unfalsifiable manner. The PH is therefore in my view not an empirical one and is simply a fixed idea.

Chapter 2 investigates feature theories, *e.g.* underspecification and feature geometry, and discusses the metatheoretical problems these framework have due to the assumption of the PH. This demonstrates that phonological processes involving 'nasality' cannot be explained by the employment of features. It follows that the standard definition of 'nasality' via the feature [±nasal] is useless. 'Nasality' and 'nasalisation' are thus phonetic, *i.e.* speech implementational or perceptional, manifestations of a phonological, *i.e.* cognitive, unit which must be established independently of phonetics.

In chapter 3, I look at the commonly held view that there is a phonetically motivated phonologically relevant link between nasality and vocalic height or consonantal place of articulation (the 'Heightmyth', 'HM'). I show that evidence put forward in support of this proposal—I discuss the historical development of French nasal vowels, French denasalisation, Chinese nasal vowels and preferential nasalisation of non-high vowels in Teke languages (Bantu) and of reconstructed °a in Romagnol dialects—does in no way argue for any phonetic motivation of the attested cases of affinity between nasality and certain vowel heights. In addition, I discuss synchronic evidence in favour of the HM and come to the conclusion that the phenomena referred to are not phonologically relevant.

All in all, it can be said that any version of the PH investigated in this thesis exhibits the same problem: no version of the PH is empirical.

Part 2 of this thesis shows in four chapters (chapters refcognitivesolution, 5, 6, 7) how a cognitive account avoids the metatheoretical problems of the phonetic approach. In addition, it introduces a new proposal in relation to the acquisitional role of phonology:

Chapter 4 provides an introduction to Government Phonology ('GP') and, more specifically, to GP's subtheories dealing with melody: (Revised) Element Theory and the Theory of Generative Constraints. This chapter demonstrates that there are languages with phonetically oral vowels which can phonetically nasalise following oral consonants. This argues in favour of a phonetics-independent cognitive motivation of nasality.

GP employs monovalent (privative) cognitve units, called 'elements', for segment-internal representations. In chapter 5, I put forward evidence for the merger of Kaye, Lowenstamm & Vergnaud's L- and N-element into one new element (new) L. The main advantages of such a move are that it helps to keep overgeneration down and that it provides the basis for a integrated account for the cross-linguistically attested phenomena of nasality-induced voicing, Dahl's and Meinhof's Law. The link between these phenomena, which were up to now considered unrelated, is, as I show, only possible if one is prepared to abandon the PH and to look for a cognitive explanation.

Chapter 6 investigates Québec French nasal vowels, Montpelier VN-sequences and English NC-clusters and proposes a unified account for them. This analysis includes a cognitive explanation of the French version of the Heightmyth, *i.e.* for the observation that French vowels may not be high. In addition, I propose an account for the neutralisation of tense-lax oppositions in Brazilian Portuguese nasal vowels next to well-formed contrasts of this kind for oral vowels. Again, only a cognitive, not a phonetic, view is able to explain the restrictions of such a vowel system.

Finally, in chapter 7, I demonstrate that the view that the PH is mistaken points to a new insight: Acoustic cues do not only contain much phonologically useless packaging in addition to phonologically relevant material, but also underdetermine the phonological representation. In other words, acoustic cues do not always contain all the information necessary to determine the internal representation of a segment. This is due to a phenomenon I have labelled 'acoustic cue overlap'. I can show for a number of Turkic vowel systems that they could not be acquired without the help of phonological processes (I- and U-harmony). Similarly, even though phonetically defined cues like 'voiced' or 'voiceless' for segments do not contain much useful information in relation to the phonological behaviour of the segments involved, there is cross-linguistic evidence for my claim that many consonant systems (including those exhibiting voiced-voiceless contrasts) could not be acquired without the helping, *i.e.* disambiguating, hand of phonology.

All in all, the cognitive approach to phonology will not only be shown to be more empirical than the phonetic approach but also to be much more insightful.

Part I

The Phonetic Approach

Chapter 1

The Nasal Fallacy

Introduction

In the first three chapters of this thesis, I argue against the assumption that phonological nasality is phonetically motivated.[1] It will follow that there is no phonologically relevant phonetic level of representation. This chapter will also provide the basis for chapter 2 in which I will explain why the assumption of a feature [nasal] (which is employed by the majority of modern phonologists to provide a phonologically relevant definition of the supposedly phonetically defined concept 'nasality') is not an empirical one.

Let me start by stating categorically that this chapter does not explain what phonological nasality is. There is, actually, no such thing. I will, however, refute the 'Phonetic Hypothesis' (henceforth 'PH'), *i.e.* the mainstream view that phonological phenomena, *i.e.* including those involving nasality, are motivated by the properties of a phonetically characterised system, *e.g.* the articulatory or auditory system.[2] I will demonstrate that theories which assume the PH must set it up in an unfalsifiable manner and that the PH can therefore not be viewed as empirical assumption.

Let me provide a brief abstract. In the first section of this chapter, section 1.1, I explain the reasons why I agree in opposition to the humanitarian Feyerabend [161] with Popper's [393, 395] arguments in favour of an empirical and evolutionary approach to knowledge. Note that this does not prevent me from supporting Feyerabend's view that science, as it is practised within the socio-political structure pre-

[1] An ealier version of this chapter can be found in Ploch [382].

[2] Even though many of the arguments against the PH provided in the following are derived from a discussion of phonological frameworks motivated by articulatory phonetics, it is not my intention to suggest that all phonetically motivated phonological theories are of this kind. The reason for the apparent bias is that since Chomsky & Halle [97] the phonological mainstream has rarely if ever been concerned with the acoustic properties of speech.

scribed by public universities, is *de facto* about myth creation and about real relationships between real people, and not about being empirical. Then, in section 1.2, I support Kaye's [253] arguments against the assumption of the PH. Section 1.3 discusses the main strategies employed by supporters of the PH in order to maintain it in spite of the evidence against it; the examples I look at are Lass [286] and Archangeli & Pulleyblank [17]. In section 1.4, I examine findings by Entenman [153] (1.4.1), Vaissière [482] (1.4.2), Ladefoged [283] (1.4.3), Huffman [226] (1.4.4) and Brun, Spencer & Fourcin [72] (1.4.5) which make it obvious that the general assumption that there *is* a phonetic, *i.e.* independently and scientifically established, definition of nasality is ill-founded. In section 1.5, I argue against the analyses of nasality phenomena within articulatory phonology as proposed by Browman & Goldstein [69] (1.5.1), within Kawasaki's [248] experimental phonology (1.5.2), Hawkins & Stevens's proposal of 'acoustic correlates' (1.5.3) and Clements & Hertz's 'integrated' approach (1.5.4). While Browman & Goldstein assume an articulatory version of the PH, Kawasaki, Hawkins & Stevens and Clements & Hertz base their respective frameworks on acoustic phonetics; however, all approaches do presume phonetics to be relevant to phonology and, as I will demonstrate, are therefore simply circular/unfalsifiable or make fundamentally wrong predictions about the data they try to explain.

My conclusion will be that the PH is irrelevant to the study of nasality (and phonology in general). In other words, a phonological account of nasality cannot successfully make any reference to phonetics.

1.1 In defense of method, empiricism and falsifiability

In this section, I will outline briefly why in my opinion there can be no *empirical* science without critical discussion based on "the method of bold conjectures and ingenious and severe attempts to refute them" (Popper [393, p. 81]. To begin, let me state clearly that I do not intend to legislate here. That is to say that I do not want to claim about any particular scientific method that it is the only possible one. What I would like to point out is that Popper's findings are a tool which, when applied, makes it more difficult for scientists to delude themselves into thinking that their analysis is supported by evidence simply because they have found a set of hypotheses that predicts a certain set of data. Furthermore, it is possible to support Popper's views on empiricism without agreeing with his prescriptive proposal of moral responsibilities of the scientist.

According to Popper, "All Knowledge is Theory-Impregnated, Including our Observations" [393, p. 71]. Even 'objective' knowledge acquired via our senses is dispositional because our sense organs are the result of a trial-and-error based evolution. This is why Popper sees his scientific method as an evolutionary approach: We make a hypothesis and try to refute it. If we err, we try another hypothesis; thus our knowledge is increased:

> "The difference between the amoeba and Einstein is that, although both make use of the method of trial and error elimination, the amoeba dislikes to err [it dies] while Einstein is intrigued by it: he consciously searches for his errors in the hope of learning by their discovery and elimination. The method of science is the critical method" [393, p. 70].

In this context, it is important to mention that if we cannot refute a given theory or set of assumptions, this does not verify that theory or set. One of Popper's important findings is that, for a given set of assumptions, there is an asymmetry between verification and falsification: while a set of assumptions can never be proven to be true, it is theoretically possible to prove (falsifiable) assumptions wrong. Of course, in order for us to try to refute a set of assumptions, the set in question has to be set up in a refutable, *i.e.* falsifiable or testable, manner. If it is not, *i.e.* if the set has been immunised against refutation [*ibid.*, p. 39] by making it unrefutable, *i.e.* unfalsifiable and untestable, it is not an empirical set of assumptions. Consequently, says Popper, (empirical) scientific hypotheses must be falsifiable. The following quote should clarify this:

> ". . . every scientist who claims that his theory is supported by experiment or observation should be prepared to ask himself the following question: Can I describe any possible results of observation or experiment which, if actually reached, would refute my theory? If not, then my theory is clearly not an empirical theory. For if all conceivable observations agree with my theory, then I cannot be entitled to claim of any particular observation that it gives empirical support to my theory" (Popper [395, p. 88]).

Furthermore, I agree with Popper's view that "the aim of science is increase of verisimilitude" [393, p. 71]. Popper distinguishes *truth* from *truth content*, *i.e.* the class of all statements which follow from a statement [*ibid.*, p. 48]. While, for example, tautologies like *Tables are tables* are indubitably true, their truth content is zero. The truth of scientific theories, like Einstein's relativity theory, can never be verified and, since theories (usually) entail false statements, *i.e.* problematic data,—and if they appear not to, they cannot be proven not to—they are (usually) to some extent untrue or, in other words, have some amount of falsity content, *i.e.* a higher falsity content than indubitably true tautologies. Consequently, when Popper says that science is about the search for truth [p. 44], he is not interested in truth but in truth content. Also, since competing theories can often account for the same amount of data, *i.e.* have identical truth contents, but differ with respect to the amount of data problematic within their (respective) approaches, *i.e.* with respect to their falsity contents, Popper argues for a concept as the aim of (empirical) science which encompasses both the demand for relatively high truth content and relatively low falsity content: this concept is *verisimilitude* (relative 'close-to-truth-ness').

Let me also point out that Popper does not claim that a theory which at some stage of its development is unrefutable is a useless theory: "It should be made quite clear that there are many examples in the history of science of theories which at some stage of the development of science were not testable but which became testable at a later stage ... This should be a warning to those who are inclined to say that nontestable theories are meaningless" (Popper [395, p. 88]). I consider it futile to discuss whether or not a theory which is not testable and therefore not useful at present should not be regarded as a useless theory because it *may* become testable and useful in future. Let us however keep in mind that usefulness specific to future versions of a set of hypotheses is the ideal tool for theoreticians who want to adhere to some "fixed idea" (Stirner [463, 462]); they can always claim without or with only little evidence that their theory is useful now because it *might* become useful—*i.e.* testable—at some later stage.

Let me add here that I am aware of Feyerabend's *Against Method* [161], *Farewell to Reason* [162] and *Three Dialogues on Knowledge* [163]. I am of the opinion that, opposed to Feyerabend's conclusions, Popper's methodological view does not imply restrictiveness, authoritarianism and/or pedantry ("Popper is not a philosopher, he is a pedant—this is why the Germans love him so" [163, p. 50]) and can still in many ways be interpreted as Dadaist type of anarchism as defined by Feyerabend (*cf.* [161, in particular, p. 21n]), *i.e.* as anarchism which detests "Puritanical dedication and seriousness" [*ibid.*] and that is "utterly unimpressed by any serious enterprise" [*ibid.*].

Even though Feyerabend never discusses this, it is possible to approach science rationally without being puritanically serious about it and without denying the validity of Feyerabend's claim that science (as it is commonly practiced) is the most dogmatic religious institution and that science must be separated from the state [*ibid.*, chapter 18]. One can agree with Popper's views on empirical science without agreeing with the legislative connotations they may or may not have for some or may even have for virtually all researchers. Feyerabend simply confuses Popper's evolutionary approach, *i.e.* the combination of conjectures and attempts at refuting them, with that which when practised is commonly referred to as 'scientific research'. Consider the following quote from Feyerabend's *Third Dialogue* [163, p. 140] ('B' appears to portray Feyerabend's view):

> "*B:* Historians have studied the actual sequence of the events that led from a scientific problem to a conjecture to ... the projection of results and the final acceptance of the results by ... scientists. ... Proceeding in this way they discovered that the process contains much that is tentative, not explicit ...
> *A:* Which is exactly what Popper says. He says that when dealing with a problem we make conjectures, that the conjectures are tentative, that we revise them on the basis of refutations.
> *B:* Which is exactly what does *not* happen at decisive junctures of scientific research. Conjectures there may be, but many of them are unconscious and they are changed and modified without any explicit discussion, simply as part of an overall process. And note, the adaptation does not involve a mystical entity, called 'objective reality', but real relations between people and things."

Apparently, B thinks that *de facto*, scientific research is dependent on the historical context ('real relations between people and things'). I agree. However, I do not see in what way this dependency of research on the historical context argues against Popper's proposal of objective knowledge. The question whether falsifiability is an obligatory condition on empiricism is orthogonal to the question what the practice of research actually looks like. And even if we assume that any hypothesis is in some way influenced by the historical context in which it is proposed or used, we can still compare any two hypotheses (both dependent on the historical context) with respect to the degree of falsifiability they exhibit and thus decide which one is more empirical. Let me add though that a similar fallacious syllogism (of the metatheory on scientific empiricism and scientific practice) can be found in Popper's own writings.

So Popper does not only claim that it is possible to be rational and that falsifiability and verisimilitude are notions relevant in this context but also that we do "not only ... reason rationally, ... but that we also act rationally" [393, p. 95]. However, while accepting Popper's proposal of falsifiability as criterion of demarcation between empiricism and non-empiricism, it is possible to disagree with his assumption that humans (usually) act rationally. Stirner, for example, has the following view:[3]

> "Do not think that I am jesting or speaking figuratively when I regard those persons who cling to the Higher, and (because the vast majority belongs under this head) almost the whole world of men, as veritable fools, fools in a madhouse." (Stirner [462, p. 55])

Importantly, it is possible to agree with both Stirner's opinion about the majority of people—*i.e.* that they are irrational fools—and with Popper's definition of empiricism, *i.e.* his criterion of demarcation between science and pseudo-science/myth formation. Consequently, I maintain my support of empiricism as criterion of demarcation between empiricism and non-empiricism. It may well be part of such a view to congratulate any new idea whether or not one can build an explanation on it. However, given two competing hypotheses, if one wants to decide which one of them works best *at present, given the evidence (as one is aware of it)*, this decision must be an empirical one if it is to be an objective decision. Otherwise, the resulting judgement will be imagined and, if upheld nevertheless, sacred.[4] Let me also point out that this 'call for verisimilitude' is not intended as a moral calling for scientists. My claim is simply a) that empiricism is humanly possible—which is not the same as to say that empiricism is commonly practised at universities, that empiricism is possible for all humans or that for those to whom it is possible it is so to the same extent—and b) that there is no empiricism without falsifiability and 'ingenious attempts' at refutation of a given hypothesis.

To sum up, I have argued in this section that, in line with Popper's evolutionary

[3]In the German original: "Denke nicht, daß Ich scherze oder bildlich rede, wenn Ich die am Höheren hängenden Menschen, und weil die ungeheure Mehrzahl hierher gehört, fast die ganze Menschenwelt für veritable Narren, Narren im Tollhause ansehe." [463, p. 46]

[4]For those who would want to discard objectivity as a relevant concept simply because it is not possible to be 100% objective, let me add the following: Since this kind of objective knowledge is seen as an approximation to truth (veri*similitude*), not truth itself, it would be more precise to say that the more empirical a decision is the more objective it is, even though 100% objectivity may not be an achievement possible to humans. Similarly, the less empirical/objective a decision in favour of a given hypothesis is the more the assumption of the correctness of that hypothesis is imagined and the hypothesis itself sacred.

approach to epistemology, the method of critical discussion of competing bold hypotheses in connection with ingenious attempts at refuting these hypotheses (to use Popper's jargon) is a prerequisite for empirical science. In other words, explanations which are set up in an unfalsifiable manner are not empirical; since any particular explanation which only *may be* or *may become* relevant is more likely to be irrelevant than relevant, non-empirical assumptions can*not* be considered explanatorily relevant and, even though they might be inspirational and thought-provoking and may well be worth being kept in the back of one's mind because they might turn out to be empirically useful later, they are, at a given time, explanatorily useless (which is not equal to scientifically useless). On the basis of the findings of this section, I will in the following go on to show that the assumption that the phonology is motivated by or grounded in the phonetics (however defined) is not falsifiable, thus non-empirical and therefore (at present) explanatorily useless.

1.2 The irrelevance of the articulatory system

The argumentation of this section will mainly follow Kaye's argumentation against the widely accepted proposal that phonological phenomena are articulatorily motivated [253, in particular pp. 42–49]. At the end I will outline an application of Kaye's arguments to frameworks which either assume an acoustic version of the PH or both an articulatory and an acoustic version. Since large parts of the phonological mainstream insist on the inclusion of the PH in the set of assumptions made by their (respective) frameworks, it will follow that this majority is predominantly concerned with a myth[5] and the reinforcement of a fixed idea.

In his discussion, Kaye uses both phonological processes and linguistic change as evidence for his claim that phonological phenomena are *not* based on properties of the articulatory system. This view currently defines an essential part of the conglomerate of theories labelled 'Government Phonology' ('GP') and sets it apart from most other phonological theories. Kaye points out that the articulatory version of the PH is based on the claim that phonological processes involve an increase in 'ease of

[5]Feyerabend [161, pp. 295ff.] argues that certain "features ... refute the assumption that science and myth obey different principles of formation" [p. 297]. I cannot find a single argument in Feyerabend's book in favour of this statement. Feyerabend also claims that Western science, the peak of the *mysteries* of Western rationalism, is used as a bonus for the most intelligent members of non-Western tribes as part of their physical and intellectual suppression by the Westerners [p. 299]. I agree. This, however, contains no argument in favour of the claim that *empirically* established knowledge, *i.e.* an approximation to truth, and myths obey the same principles of formation. Myths are neither trial-and-error based nor are they testable.

articulation'. This assumption predicts the phonetic and phonological convergence of all human languages over time. Unfortunately, this assumption does not hold true: no such cross-linguistic convergence can be observed. For most proposed phonological processes there is a vast number of languages where they do not occur and where there is no evidence that would suggest the ongoing or future acquisition of these processes.

To counter Kaye's argument, one could assume that a given process is not necessarily equally costly to speakers of different languages. The PH could be upheld and phonology would be mainly concerned with language-specific studies. Since any human being can learn any human language natively without marked differences *e.g.* across races, nations or continents, this assumption is unfounded. Furthermore, since no-one has been able to successfully formulate a theory of language-specific scales of muscular effort, this assumption is also not falsifiable—and therefore, in my view, a fixed (dead) idea.

A supporter of the PH could also try to avoid the prediction of cross-linguistic convergence by claiming that the expected convergence has not happened yet but will, that it happens so slowly that linguists have not been around long enough to study them or that the observable phonological phenomena are not as drastic as the ones yet to come. Since there *are* phonological processes, one would still expect these to provide evidence for cross-linguistic convergence—which they do not. Furthermore, there are limits to what phonological processes in natural language may look like. Consequently, it does not seem reasonable to expect future phonological processes which are unlike those of the past or present if there is no evidence for such an expectation. Additionally, linguistic change happens quite rapidly. Kaye points to the example of the modern Romance languages [*ibid.*, pp. 45f.] which evolved from a variety of Latin in less than 2000 years. If phonological processes occurred to ease articulation, a given set of Romance proto-languages should have shown signs of convergence since they split off from Latin as opposed to the observable divergence over time. I would like to add here that for this example to work one has to assume that the split of the Romance languages away from Latin was not phonetically motivated. If it had been phonetically motivated, it would remain completely unclear why they should have split off Latin in many different ways in the first place. Also, any proposal which asks scientists to *believe* that something that has not happened will happen, even though there is no evidence for such a proposal, also asks them to be religious and essentially unscientific.

A further argument against the PH is the fact that it predicts that the input of a synchronic or diachronic phonological change in one language should not be the result of a process in another. To use Kaye's example [p. 46)], the Latin sequences kt and pt changed to tː ⟨tt⟩ in Italian. Compare Latin ⟨doctor⟩ 'teacher', ⟨adoptare⟩ 'choose' with Italian ⟨dottore⟩, ⟨adottare⟩. The PH would explain this by saying that it requires less musculatory effort to pronounce tː than kt or pt. This explanation is proven wrong by all processes which create kt or pt where there was no consonant sequence before the application of such a process. So if kt turns into tː in Italian due to phonetic reasons, Classical Arabic kataba ('he wrote') should not change to ktɨb in modern Moroccan Arabic due to phonetic reasons. Similarly, processes of backtracking or rule inversion should not occur. For example, varieties of Yiddish which had previously introduced final obstruent devoicing—presumably to ease articulation—should not eliminate it later on. As Kaye points out [*ibid.*, pp 47f.], Weinreich [488] shows successfully that this is precisely what happened though.

To counter Kaye's argument, one could refer to examples of backtracking in biological evolution, *e.g.* the loss of flight, which *do* occur. However, such examples are always cases of adaptation to a changing environment. In order to make the PH work, one would have to propose that a phonological process or change, *e.g.* the introduction and subsequent elimination of final obstruent devoicing, is an adaptation to changes in the environment, *e.g.* the climate, geographical location, ways of collecting food, *etc.* Since there is no evidence for any link between properties of the environment and those of phonological processes, Kaye concludes that phonological processes are not adaptive and that therefore cases of backtracking do provide evidence against the PH.

One could also try to maintain the PH at an individual level. In this case one would predict correlations between the number of phonological processes and the physical state of the individual. For example, the speech of tired people should contain more phonological processes than the speech of people less tired. Again, since there is no evidence for any such correlation, the PH is extremely unlikely to be true.

Kaye finishes his discussion of the articulatory hypothesis by pointing out that phonetic processes do exist [*ibid.*, p. 49]. So a d will sound slightly different before i than before a in all languages which have d, i and a. It is characteristic though for such phonetic processes to be "omnipresent" [*ibid.*] across languages, which is why they cannot be used to differentiate one phonological system from another or to establish the phonological part of Universal Grammar.

Let me add here that an acoustic version of the PH, *e.g.* Stevens & Blumstein [455], would claim that phonological phenomena are motivated by perceptual reasons. Since all languages can be learnt natively by everyone, it is reasonable to assume that the acoustic system of all humans is identical—linguistically speaking. In line with the articulatory hypothesis, this would predict the universal convergence of all human languages, which, as pointed out by Kaye, completely eludes detection. Moreover, all of Kaye's arguments against the articulatory hypothesis as presented above work equally well against the acoustic hypothesis. For example, if Latin kt and pt changed to tː in Italian because of perceptual reasons—and it is doubtful whether any properties of tː are perceptually more adequate than those of kt or pt—Classical Arabic <u>kat</u>aba should not have turned into modern Moroccan Arabic <u>kt</u>ɨb due to perceptual reasons. This is not to say that there is no relation between the acoustic signal and phonological structure. However, in order to avoid circularity, what is considered contrastive within the phonology must be established independently of these correlates.[6]

In addition, if one were to assume the articulatory and the acoustic version of the PH simultaneously, it would be necessary to establish independently what types of phonological phenomena would be predicted by the acoustic hypothesis, by the articulatory one or by both. To state without such independent evidence that both are necessary, that "different languages may apparently [*sic*] use either articulatory or acoustic features (or both)" (*cf.* Lass [286, p. 99]) and that what kind of feature is actually used is a "matter for empirical investigation" [*ibid.*, p. 100] ensures that both the articulatory and the acoustic version of the PH are unfalsifiable, which results in an unscientific framework.

To sum up, there is no evidence for the proposal that phonological phenomena are caused or motivated by or based on properties of the articulatory or any other phonetically defined system, and all the available evidence is contrary to the PH.

1.3 Maintaining the Phonetic Hypothesis

In light of this evidence it is important to look at how supporters of the PH maintain their position. There are two main strategies in past and current literature both of

[6]For a general discussion of invariant acoustic correlates of phonological contrasts, *cf.* Stevens & Blumstein [456] and Williams [496]; for invariant cues for place of articulation in stops, *cf.* Stevens & Blumstein [455]; for acoustic correlates of the distinction between nasal and oral vowels, *cf.* Hawkins & Stevens [209].

which ensure that the PH is set up as an unfalsifiable dogma which is simply accepted and rarely (if ever) questioned: denial and flexibility. I will first introduce the two strategies involved and then provide one example to be found in the literature for each of them (Lass [286] for denial, Archangeli & Pulleyblank [17] for flexibility).

1.3.1 Strategy 1: denial

Phonologists who make use of the strategy of denial ignore counterexamples to the PH while they try to discover more and more cases where the PH does predict observable phenomena. For example, Kenstowicz [271] and Lass [286] provide an introduction to feature systems based on phonetically defined properties and a vast number of processes accounted for by these features. However, not once do they discuss the problem that for any language in which a given process does occur one can virtually always point to a language where it does not. Since all humans can learn any language natively they must have the same articulatory system. It remains therefore unclear why it is simply not a problem for Kenstowicz and Lass that most of the phenomena predicted by their theories do not occur in even half of the world's languages. To counter this, one could point to the concept of parametric variation within Universal Grammar. This would mean that cross-linguistic differences are explained by different cognitively, not phonetically, defined systems. This, however, does not solve the problem: within such an approach the PH is never tested (*cf.* strategy 2, below) and all the available evidence can by definition only be in favour of it. Whenever the strategy of denial is used, the PH is consequently not an empirical assumption. I would like to suggest that the PH is rather a fixed idea and therefore a psychological and/or socio-political phenomenon. Power is sweet.

1.3.2 Strategy 2: flexibility of applicability

Strategy 2 is achieved by making the conditions on the applicability of the PH flexible. In a framework of this type, phonological processes are explained by the manipulation of cognitive yet phonetically motivated units. However, whenever phonetic measurements cannot explain observable patterns, other 'phonological' evidence which *cannot* be motivated by phonetics is employed to account for the problematic data. Since in such a theory there is no independently established criterion according to which one could decide when not to apply the PH, this flexible approach to falsifiability enables its practitioners to assume the PH without having to set it up in a scientific manner.

A classic example can be found in Archangeli and Pulleyblank (henceforth 'AP') [17]: AP's "Grounded Phonology" [*ibid.*] is a feature theory. Features are called "F-elements" which form the "primitives of a formal model of phonological feature content" [p. 47]. Recognising the problem that an unconstrained combination of these F-elements would result in far too many theoretically possible phonological expressions [p. 167],[7] AP propose "grounding conditions" [*ibid.*], *i.e.* "conditions used in natural language [which] directly reflect physical correlates of the F-elements involved. Thus, such conditions are physically *grounded*" [*ibid.*]. As example they point to the F-element [+nasal] which in most cases co-occurs with the F-element [+voiced]. The grounding condition (in a simplified form) expressing this would look like: "*If [+nasal] then [+voiced]*" [p. 168]. This condition is physically grounded in that it is based on the observation that lowering of the velum—resulting in air passing through the nasal tract—creates a situation in which there is virtually no build-up of pressure, which in turn is "amenable to periodic vocal cord vibration" [*ibid.*]. Consequently, a grounding condition like "*If [+nasal] then [−voiced]*" [*ibid.*] could in AP's view not be part of a sensible phonological theory.

The main problem with Grounded Phonology is that one of its fundamental assumptions, *i.e.* the grounding hypothesis, is not falsifiable: Even though any phonological process is 'grounded' in the universally shared articulatory system of humans, most of these processes do not occur in most other languages. Consequently, this non-occurrence constitutes a serious counterexample for Grounded Phonology. However, AP simply propose that the predictions of their grounding hypotheses can be violated. Let me provide a few examples: Nasals are voiced in most cases, and this can supposedly be phonetically motivated. Consequently, voiceless nasal stops ([+nasal, −voiced]), which do occur (*e.g.* in Angas, Kwangali, Burmese or Comaltepec Chinantec),[8] have to be allowed for somehow, and the stipulation allowing for such voiceless nasal consonants (henceforth 'NC') would have to be 'phonological', where the phonology involved would in such a case have to be independent

[7] *Cf.* section 2.1 in chapter 2.

[8] Angas is a Chadic language spoken in Nigeria, *cf.* Burquest [74]. The voiceless nasal stops in Angas discussed here only occur in domain-final position of an utterance-final domain—"in the coda slot of utterance-final syllables" [74, pp. 37, 38, 39, for devoiced m̥, n̥, ŋ̊, respectively]. Even if phoneticians find an 'explanation' for the occurrence of devoiced nasal stops in domain- and/or utterance-final position, this would still not tell us anything about why many other languages do not devoice nasal stops in this context and would uncover this explanation as arbitrary and therefore flawed. Kwangali is a Southwest African Bantu language. For a discussion of nasals in Kwangali established as voiceless via kymography, *cf.* Dammann [125]. *Cf.* Silverman [449] pointing to Okell [354] and Dantsuji [126, 127, 128] for Burmese and to Anderson [4], Anderson, Martinez & Pace [5], Pace [359] and Silverman [448] for Chinantec.

of the very same phonetic properties which are assumed to motivate phonology. To give another example, why does roundness harmony ('U-harmony' in Government Phonology), as it can be found in a number of Turkic languages (*cf.* Charette & Göksel [86, 87]), not occur in all languages? Also, why is U-harmony not subject to the same constraints in all languages in which it operates?

In AP's view, this problem is solved by claiming that the phonetic motivation for one grounding condition can be more or less strong than the motivation for another. Consider their "Grounding Conditions" [17, p. 177]:

(1) *The Grounding Conditions*

I. Path conditions invoked by languages must be phonetically motivated.

II. The stronger the phonetic motivation for a path condition ϕ,

a. the greater the likelihood of invoking ϕ,

b. the greater the likelihood of assigning a wide scope to ϕ within a grammar,

and *vice versa.*

This is to say that whenever a phonological process operates in one language under certain constraints but is not observable in another, AP 'explain' such cross-linguistic differences by saying that the phonetic motivation for a given process is strong enough to invoke a path condition in a language in which that process occurs while the (presumably identical) phonetic motivation for the same process is not strong enough to invoke the same path condition in a language in which that process does not occur (Grounding Condition IIa). Obviously, it remains completely unclear how "likelihood" and "strength of phonetic motivation" could be measured in a scientific way, and not surprisingly, AP have to add that it is quite unclear to them too. So they admit that "Grounding Condition II makes predictions about cross-linguistic and intralinguistic *tendencies*" [p. 178, italics mine], that the "strength hypothesis is only half developed" [*ibid.*], if not half-baked, and that they "leave largely unaddressed the question of whether phonological robustness correlates straightforwardly [*sic*] with phonetic strength" [*ibid.*]. Unfortunately, this approach results in a theory that is based on an assumption, in this case the Grounding Hypothesis, whose numerous counterexamples are explicitly allowed for. In other words, empirically established evidence for the PH which virtually all analyses within Grounded Phonology are dependent on is only of secondary importance to AP.

Furthermore, cross-linguistic differences are also predicted in AP's theory by "parametric rules" [pp. 283ff.]. AP propose four types of parameters: Function (INSERT or DELETE), Type (PATH or F-ELEMENT), Direction (LEFT TO RIGHT or RIGHT TO LEFT) and Iteration (ITERATIVE or NONITERATIVE). This, however, does not eliminate their basic problem. Even though this system can account for, let us say, ATR-harmony in Yoruba ([−ATR]-harmony for AP [p. 14]), it remains unexplained why other languages have either a different type of ATR-harmony or no such harmony process at all as long as one assumes that ATR-harmony is grounded in phonetics. Why is the phonetic motivation to 'do' [−ATR]-harmony less strong for native English speakers than for Yoruba speakers? Also, why does the phonetic motivation for [−ATR]-harmony—presumably equally strong for Yoruba and Wolof speakers—result in an [−ATR]-association taking place from the right edge in Yoruba but from the left edge in Wolof [p. 298]? If [−ATR]-association is grounded in universally shared phonetics, such differences across languages are predicted to be nonexistent. Unfortunately, the proposal of parametric rules does not solve this problem but merely names it. AP, like most linguists, simply assume the relevance of speech organs because *some* phonological facts can be accounted for this way. Whenever the PH makes wrong predictions AP allow for this via parametric variation in speech organ usage and in the applicability of their grounding conditions, *i.e.* in the applicability of the PH. Consequently, Grounded Phonology provides an explanation of phonological phenomena that could never be falsified which in turn is a hallmark of mythology and other psychological (non-empirical) tools.[9]

As I have shown in this section, the PH can only be maintained within a phonological theory by making it unfalsifiable. Supporters of such a framework achieve this by ignoring counterexamples of the PH and/or by adopting a rather flexible approach to its applicability.

1.3.3 Degrees of falsifiability: disproving *vs.* not applying an assumption

Based on their observation that the "overwhelming majority of the sound patterns found in natural languages reflect phonetic naturalness", Dolbey & Hansson ('DH') [140, handout, p. 1] gave a talk in which they proposed that this phonetic naturalness is not caused by any synchronic phonetic system but by diachronic processes which—opposed to synchronic phonology—*are* motivated phonetically and

[9]Please note that I would not want to imply that all psycho-tools are useless. I am merely saying that they are not empirical but that science, and that is its great advantage, can be.

thus phonetically natural. After their talk, I suggested to them that even this exclusively diachronic version of the PH is not set up in a falsifiable manner. Andrew Dolbey's answer was that he disagrees with this view, *i.e.* he does consider their approach to be falsifiable—and thus empirical—, since any diachronic process going *against* the phonetically natural direction would prove their assumption of the relevance of phonetic naturalness in relation to diachrony wrong. Since there are, in his opinion, no such counter-natural sound changes, their view is simultaneously falsifiable and not proven wrong, and therefore a valid explanatory candidate. Note that his answer contains two statements: Firstly, there are no counter-natural diachronic sound changes. Secondly, DH's assumption that phonetic naturalness is encoded diachronically is set up in a falsifiable manner.

Let me start with his first statement. In spite of what Dolbey seems to think, there *are* counter-natural diachronic changes—unless he immunises his statement against refutation by strategy 2 (flexibility), *i.e.* by having an auxiliary hypothesis ready which 'accounts' for all otherwise counter-natural changes. As pointed out in section 1.2, Weinreich [488] shows that certain varieties of Yiddish which had introduced final obstruent devoicing got rid of it later on. So if the introduction of final obstruent devoicing were phonetically natural, the elimination of the same phenomenon cannot be considered phonetically natural by the same theory—if 'phonetic naturalness' is supposed to mean anything. Alternatively, if the elimination of final obstruent devoicing is predicted by DH to be phonetically natural, its introduction cannot be.

Dolbey's second statement was that the proposal that phonetic naturalness is encoded diachronically is set up in a falsifiable manner. I find it surprising though that DH apparently need no independently established theory on what is phonetically natural. In other words, as long as they make no predictions on whether the introduction or elimination of a sound change is phonetically natural, DH get lucky whenever only the introduction *or* elimination of a process/constraint can be found: whichever version is attested can then be considered phonetically natural without any independent evidence for the natural status of the change in question. Such independent evidence would have to be established independently of attested diachronic changes since the attested changes are supposed to be predictable on the basis of the phonetics. In addition, the predictions of this kind of evidence, *i.e.* that speakers/listeners try to get closer to or avoid certain sound patterns, could not be allowed not to bear out in an unpredictable number of languages.

DH could try to counter this by saying that it is possible to decide independently, *i.e.* based on theories of articulatory effort or acoustic discriminability, what constitutes a phonetically natural phenomenon. Since articulatory effort and acoustic discriminability are opposing forces, a theory of phonetic naturalness making use of both of these types of forces can regard the introduction and elimination of the same phonological phenomenon or diachronic change as phonetically natural. For example, the introduction of umlaut can be seen as phonetically natural if viewed as articulatory assimilation; the elimination of umlaut, on the other hand—as well as the cross-linguistic markedness of umlauts (round high front vowels) as opposed to the more common round high back vowels—, could be viewed as acoustically natural due to increased degree of perceptual discriminability. Even if DH's independent theory on phonetic naturalness were either only articulatorily *or* acoustically based, they would still have the same problem which, to my knowledge, all phonetically motivated theories have. It is within such approaches not possible to predict on the basis of the phonetic properties of the sounds involved in a diachronic (or synchronic) change what phonetic property or set of such properties is diacronically (or synchronically) relevant. This always has to be decided independently of the very phonetic properties which are considered to motivate the change in the first place (*cf.* Ladefoged [283] on the non-existence of a well-defined phonetic framework, section 1.4.3), and after having checked what diachronical change—which is supposedly motivated phonetically—actually occurs.

This means that even if it were true that there are no counter-natural diachronic developments, the naturalness of many examples of sound change is a concept so vaguely defined that it is in fact unfalsifiable. So DH's psychological trick which allows them to propose phonetic naturalness as a relevant concept for an explanation of diachronical changes relates to the difference between proving an assumption wrong, on the one hand, and applying it to only a limited number of cases, on the other. In other words, while DH's assumption of diachronical phonetic naturalness *could* indeed be falsified by non-natural sound changes, it is still possible for DH to always not apply their naturalness assumption when its predictions do not manifest themselves, *i.e.* when there is no evidence which would suggest that a process which is supposedly natural in one language does not occur in the development of numerous other languages. DH's naturalness hypothesis—even though in principle falsifiable by certain evidence—is still in practice not tested because no number of languages in which a given 'natural' process is not attested would disprove the natural state of that process (strategy 2: flexibility of applicability). While the non-existence (or rarity) of counter-phonetic changes is evidence for the claim that phonology, diachronic change and phonetics are linked in some way, this link does in no way indicate that the phonetics involved motivate diachronic changes or the phonology; this would have to be established independently, and this is precisely what DH do not do. They make the same mistake that the prosecution in any whodunit movie makes: The mere fact that it is possible to come up with a story which links the accused to the crime does not make the accused responsible; the link might as well be accidental, a by-product or exhibit the reversed cause-effect directionality. So why do DH not discuss the possibility that it could be the phonology involved in synchronic or diachronic phenomena which motivates the phonetics? In my opinion, the problem that DH can not decide independently how phonetic naturalness could be defined in a phonologically relevant way makes their whole approach far too slippery to be still considered empirical. Note that I am not making any prescriptions here. Dolbey & Hansson can do what they want.

Let me also point out that even if DH's hypothesis were more testable, it would remain completely unclear why they assume that the study of diachronic changes is a core area of phonology [140, handout, p. 7] (*cf.* this volume, sections 3.2.2 and 3.2.3).

I conclude that DH provide no evidence for their claim that there are no phonetically counter-natural phenomena, and even if they claim that the majority of diachronic changes are phonetically natural, they are only able to do so because, due to the non-existence of an independent criterion for phonetic naturalness, many a change which does occur can then, as part of a somewhat circular argument, be assigned the label 'phonetically natural'. The possibility that any attested case of phonetic naturalness could—along the lines of Kaye's argumentation (*cf.* section 1.2) and the Government Phonology based view presented in part 2 of this thesis—also be due to a link between phonology and phonetics where the phonology exists independently of the phonetics and is only part of what motivates the phonetics is ignored by DH. In my opinion, phonetic naturalness is not only not encoded in synchronic but also not in diachronic phonology.

1.4 Phonetic definitions of nasality

Since the PH forms an intrinsic part of mainstream phonology, one would expect that there actually *is* a phonetic definition of widely accepted 'phonetically defined' concepts like nasality. In this section I will first look at Entenman [153] who shows some of the difficulties in providing a phonetic definition of nasality.[10] Subsequently I will discuss Vaissière [482], Ladefoged [283], Huffman [226] and Brun, Spencer & Fourcin [72] whose findings (in my view) show that nasality can neither be defined via reference to velum or velopharyngeal opening (Vaissière) nor in terms of nasal airflow (Huffman) and that the existence of a well-defined phonetic framework as basis for phonology is a myth (Ladefoged).

1.4.1 Entenman (1976): the problematic status of the concept 'nasality'

1.4.1.1 Acoustic definitions

In a chapter on the phonetics of nasality, Entenman points out that it is quite difficult to define nasality in acoustic terms [153, pp. 28–42]. So there is no acoustic feature that when added to those of an oral vowel (henceforth 'OV') produces a nasalised version of that vowel. For example, nasalisation of a vowel often leads to a modified perception of its quality. According to House & Stevens [225] (measuring vowels produced by an electrical voice tract analog), Formant 1 shifts up in nasalised vowels, but more so for [i] than for [a]. Fant [157] and Ohala [347] take this as evidence for the claim that low vowels are more difficult to nasalise (perceptually) than high vowels. However, Dickson [135] cannot find this raise in F1 bandwidth consistently while Delattre [131] finds it in English and Portuguese but not in French.

Nasality can also not be defined as lowering of F1 intensity. Even though nasal vowels (henceforth 'NVs') may have lower F1 intensity than oral ones (*cf.* Björk [49]), this intensity loss, as Dickson [135] shows, is not consistent. Also, Hattori, Yakamoto & Fujimura [207] cannot find evidence for it at all.

Furthermore, Schwartz's [442] research reveals that many of the acoustic characteristics of NVs could be obtained by means other than opening of the velopharyngeal port. It is therefore often unclear whether acoustic data which could indicate nasalisation actually does so.

In light of this evidence it can be said that neither a raise in F1 bandwidth, lower-

[10]Entenman points to a number of challenging arguments against a phonologically relevant definition of 'nasality' motivated by phonetics. These arguments, however, do not lead him to the conclusion that phonology is independent of phonetics. In spite of the evidence against the PH collected by himself he unfortunately chooses to ignore this evidence. So whenever in this section I come to the conclusion that evidence provided by Entenman is contrary to the PH, this is my conclusion, not Entenman's. Note also that in this section there is only space for some of Entenman's points.

ing of the intensity of Formant 1 nor any other measurable variation can provide an adequate definition of phonetic nasality, *i.e.* an account on the basis of which assumed phonological nasality could be predicted accurately.

1.4.1.2 Articulatory definitions

The second problem Entenman [pp. 42–48] discusses is that there is also no easily available definition of nasality based on its physiological correlates. Opposed to the impression a student of current mainstream phonology might get, Entenman's view is that "Nasality is far more than the acoustic result of opening the velum during speech" [p. 42]. He points to the findings of Lubker & Schweiger [308] who tested whether there is a correlation between a high rate of nasal airflow and perceived nasality. Their research discredits the relevance of nasal flow rates as part of a definition of nasality because it shows that even though opening of the velum will often result in an increase in perceived nasality, 52% to 79% of the variation in perceived nasality was due to reasons other than the amount of nasal flow. For example, the degree of velopharyngeal opening needed to produce nasality is dependent on vowel quality (*cf.* House & Stevens [225]). Similarly, Bell-Berti [38] and Lubker, Schweiger & Morris [309] show that different vowel qualities correspond to different velar positions (*cf.* [153, pp. 49ff.]). To be perceived nasal, [a] needs a larger velopharyngeal opening than [i] or [u], [i] needs a larger one than [u]. It follows that nasality cannot be defined as opening of the velum.

Another articulatory influence on nasality is the shape of the pharynx. Delattre [130] found that English and Portuguese speakers produce nasalisation of a vowel simply by opening of the velum while French speakers do this by opening of the velum and simultaneous adjustment of the pharyngeal cavity by tongue retraction. This adjustment results in the creation of an additional chamber. The resonances of this chamber and those created in the nasal cavity by velum opening cancel each other out. This in turn results in a lowered intensity of Formant 1, a situation which has (unsuccessfully) been proposed as acoustic characteristic for nasality (*cf.* above). As Entenman [p. 47] points out, the argument is complicated further by Minifie *et al.*'s [327] research which shows that the tongue retraction observed in French NVs resulting in pharyngeal adjustment can also be found in low vowels. Since all French vowels are low, it is not clear whether the tongue retraction observed is a characteristic of nasal or low vowels. Consequently, tongue retraction cannot be used to define nasality.

To sum up, Entenman's research makes it abundantly obvious that (until 1976) there was no phonetic definition of nasality. However, this did not prevent him or others, including virtually all nasality experts[11] and the much appreciated team 'Chomsky & Halle' [97], to accept the PH.

1.4.2 Vaissière (1988): prediction of velum movement from phonological specifications

Vaissière tries to find a set of assumptions which correctly predicts "velum height, velum movement velocity, and timing of velum movements relative to the speech waveform and to movements of other articulators" [482, p. 124] for segments specified as [+nasal] in a "given phonological representation" [p. 125]. This research shows that the feature value setting [+nasal] alone cannot accurately predict velum height: both the maximum velum height of consonants specified as [−nasal] and the minimum height of [+nasal] consonants is dependent on the context [p. 126]. Furthermore, anticipatory velum lowering in CVN-sequences "seems to begin during or prior to the first consonant" of such sequences. This means that the presumably phonetically defined feature [±nasal] cannot adequately predict velum movement nor can velum movement adequately predict phonological 'nasality'.

Vaissière's tries to solve this problem by proposing the feature [±strong]. A consonant C in a CVN-sequence is defined as [+strong] if

1. "C immediately precedes a stressed vowel" [p. 127], or
2. "C is in word-initial position" [*ibid.*], or
3. "C immediately precedes a major syntactic boundary" [*ibid.*].

In all other cases a consonant is [−strong]. Vaissière's observation is that an oral consonant (henceforth 'OC') thus specified as [+strong] is "less likely to be influenced by anticipatory velum lowering, in comparison to an OC with the feature [−strong]" [*ibid.*]. Vaissière [pp. 137f.] concludes that a closed velopharyngeal port is the target for OCs, while NCs have an open port as target. This implementation rule is further influenced by the suprasegmental feature [±strong] which accounts for the attested relative immunity of OCs against velum lowering in certain contexts.

[11] To name just a few of these nasality experts and some of the relevant literature, *cf.* Chen [91, 92], Chen & Wang [93], Clumeck [106, 107], Ferguson [158], Hyman [233, 234], Lipsky [301, 302, 303], Ruhlen [417, 418, 420, 421], and, more recently Herbert [213] and Piggott [368, 369]. Let me add that today Piggott seems to have rejected the PH (*cf.* [371]).

Unfortunately, Vaissière's proposals display a number of weaknesses: Firstly, there is no independent evidence for the assumption of the feature [±strong]. Vaissière simply gives a label to consonants which are relatively immune to anticipatory velum lowering. Consequently, there is no explanatory power in the proposal of the feature [±strong]. Secondly, Vaissière's research makes it obvious that the phonetically defined concept 'nasality' does not correlate straightforwardly with opening of the velopharyngeal port. Nasality is not only dependent on velum height but also on the phonetic and/or presumed (phonetically motivated) phonological environment, *i.e.* on [±strong], on tongue height [p. 137ff.] and the feature [±stressed] [p. 134]. Thirdly, Vaissière tries to predict velum movement from *presupposed* phonological specifications. In other words, the supposedly phonetically motivated feature [±nasal] is assumed to be phonologically relevant without evidence to support this, *i.e.* even though there is no independent phonetic or phonetically motivated 'phonological' definition of nasality. The fact that Vaissière nevertheless assumes the phonological feature [±nasal] and thus some version of the PH shows that Vaissière treats the PH as a given which no evidence needs to be provided for. Finally, what phonological features Vaissière's predictions are dependent on varies across speakers. Vaissière's paper discusses phonetic patterns of two speakers of American English [p. 122], *i.e.* of speakers A and B. While it is necessary to refer to manner features for an adequate prediction of the movement of speaker A's velum, it is not necessary in the case of speaker B [p. 134]. Since the predictive power of Vaissière's approach does not reach beyond idiolects, it is totally inadequate as a phonological tool. To be fair, Vaissière does not try to derive phonological representations from velum movement but velum movement from phonological representations. However, Vaissière's research shows that phonological nasality—phonetically defined via reference to velum movement—does not correspond to phonetic nasality nor *vice versa*.

1.4.3 Ladefoged (1989): the non-existence of a well-defined phonetic framework

In a section on "Universal phonetics and phonology", Ladefoged [283, pp. 9ff] discusses the phonetic basis of the IPA chart.[12] Note that the symbols provided by the IPA are classified according to only *some* of the articulatory properties of the sounds transcribed: which articulatory properties are assumed to be relevant for this classification is motivated 'phonologically', *i.e.* independently of the very same phonetic

[12]The most recent version of the IPA chart is revised to 1993 and can be found in Nolan [343].

properties which are hypothesised to motivate phonology. Since I have shown above that characteristics of the articulatory apparatus are not relevant to phonology, it is clear that the IPA, when assumed to be phonologically relevant, cannot make valid phonological predictions. So Ladefoged admits that there is no well-defined phonetic framework "that allows [phoneticians] to describe linguistic sounds in terms of what are taken to be extra-linguistic categories such as voicing or nasality" [p. 12]. He also states that "there is no theoretical basis for the existing phonetic framework" [*ibid.*] because it can always be modified to incorporate contrasts yet to be discovered in some language. Furthermore, innately endowed phonetic capabilities can in Ladefoged's view not be determined independently [p. 13]. What is utterly surprising is the fact that he—together with mainstream phonology—nevertheless considers articulatory effort (which cannot be determined independently!) to be "important in the formation of phonological patterns" [p. 12]. Typically, he does not regard it necessary to motivate this assumption which makes it all too obvious that he treats the PH as a doctrine to be accepted faithfully.

I conclude that due to the highly flexible nature of the applicability of the framework inherent in the IPA (*cf.* 1.3, strategy 2), the IPA is solely a table of pre-theoretical symbols, employable by linguists to transcribe subjective sound experiences. This means that even though these characters are used by virtually all phonologists, they are mere letters and have no explanatory power; for example, the classification of [ɴ] as '(pulmonic voiced) uvular NC' contains no phonologically relevant information *per se*. Furthermore, the assumption that there is a well-defined phonetic framework which can explain or motivate phonological phenomena is a fallacy or a myth for which no-one has ever provided any evidence.

1.4.4 Huffman (1989): nasal airflow and articulatory landmarks for Nasal

Huffman tries to provide a definition of (speech implemented) nasality by establishing what constitutes a phonetic change significant enough "along a phonetic dimension" [226, p. 12] to justify the characterisation of a segment as phonetically nasal. Due to the relative inaccessibility of the velum, Huffman bases her findings on nasal airflow measurements. She shows that the definition of onset and offset of nasalisation is not a matter of identifying when nasal flow turns on and off [pp. 24ff.; *cf.* Vaissière, above]. The evidence for this stems from Moll [330], Clumeck [107], Reenen [405] and her own research [*ibid.*]: Moll finds velopharyngeal opening on

English vowels in the context of OCs. Similarly, Clumeck shows velopharyngeal opening for English low vowels in oral contexts. Reenen provides X-ray tracings of OVs of a Canadian French speaker which exhibit velopharyngeal opening. In her own research, Huffman finds "many instances of nasal flow on phonemically oral segments in oral contexts" [p. 24].

Huffman's solution is to compare nasal flow rates of nasal and oral segments in identical or at least similar contexts in order to establish an "orality threshold" [p. 30], *i.e.* a definition of "what may be considered typically oral" [*ibid.*]. Nasal flow rates above this orality threshold can then be assumed to reflect contextual (phonetic) nasalisation.

Additionally, Huffman proposes a theory of constraints on the temporal distribution of the phonetic properties which realise phonological features. This theory is dependent on a theory of the temporal location of phonetically defined targets [pp. 35ff.]. Since Huffman's nasal flow measurements show that Yoruba and Akan speakers make consistent differences in timing for [+nasal] *versus* [−nasal] segments, [+nasal] segments are assigned long targets (windows with duration), while [−nasal] ones are assigned short targets (points in time without duration) [p. 50]. [+nasal], *i.e.* long, targets are constrained by "articulatory landmarks" [p. 35], *i.e.* "a small set of subsegmental structures [which] determine the location and duration in time of targets along phonetic dimensions" [*ibid.*]. Based on this theory, Huffman establishes nasal airflow levels which "reflect target levels for nasality" [p. 49]. NCs show nasal flow rates 3–5 times as high as the rates in corresponding OCs, while NVs have nasal flow rates only 2–3 times as high as comparable OVs [p. 49]. This, so Huffman, should be part of an account of the phonetic properties of phonological nasality.

Let me now discuss the main problems of Huffman's approach. Firstly, what is considered to be phonetically nasal does *not* correspond to the presence of nasal airflow; (phonologically) oral segments may contain nasal flow. Therefore Huffman is forced to establish orality thresholds, *i.e.* limits above which phonetically defined nasal flow rates are assumed to actually reflect (phonologically relevant) phonetic nasality. Furthermore, she is not able to propose a universal orality threshold which holds for all segments specified phonologically as [+nasal] or at least one for NVs and one for NCs: Huffman admits that different speakers show different values for "the absolute amount of nasal flow present in their speech" [p. 31]. Similarly, different vowel qualities and places of articulation correspond to different nasal flow rates.

So for each speaker different orality thresholds must be established for each vowel quality and each place of articulation [*ibid.*]. Moreover, nasal flow levels in oral segments vary over time and the orality thresholds to be established must thus be allowed to vary over time too [*ibid.*]. It can thus be said that in line with Vaissière's research, Huffman's proposals are only relevant to the study of idiolects. Assuming that phonetic nasality is a universal phenomenon, it remains unclear in what way Huffman's findings about nasal flow rates bear any relevance on speech implementation and why nasal flow should not merely be considered a by-product.

The second major problem of Huffman's proposal is that it contains a circular argument. Huffman [p. 55] states:

> "Under the assumption that phonological [*sic*] feature specifications are the primary [*sic*] determinants of segment quality, assignment of articulatory landmarks should follow [*sic*] from the interpretation of specifications for one or more feature(s)."

This means that Huffman measures nasal flow rates of segments specified as such and for nasality or orality (non-nasality) by phonology. In other words, phonological segments are assumed to be phonologically [+nasal] or [−nasal] without the provision of evidence for the phonological relevance of phonetic properties. Segments thus characterised are then tested for phonetic nasality via airflow measurements. Consequently, phonological nasality is in some mysterious way based on phonetic nasality while phonetic nasality is established on the basis of phonological nasality. This is tantamount to saying that Huffman's findings are relevant if they are relevant. I therefore draw the conclusion that Huffman provides no evidence for the phonetic or phonological relevance of nasal airflow rates. Note also that in Huffman's view,

> "this method of analysis is intended as a tool for investigating details of timing of the phonetic implementation of known [*sic*] phonological feature specifications, rather than as a diagnostic of the proper characterization—phonological or phonetic—of nasalization on a segment. For instance, the orality threshold approach does not provide any *a priori* method for identifying categories [...] in degree of nasalization." [p. 33]

So it remains unclear why Huffman nevertheless bases her measurements on phonetically defined 'known' feature specifications, thus creating a circular argument.

Finally, Huffman also has to admit that the oral/nasal boundaries established via her method "will not necessarily have perceptual relevance" [*ibid.*]. This question is left to future research [*ibid.*]. Since language-specific articulatory specifications of speech have to be learnt by a child, it is necessary for that child to be able to perceive these specifications. This means that Huffman's proposals *must* be perceptually relevant in order to be linguistically relevant at all. Due to the questionable status of the perceptual relevance of her findings, it unfortunately remains open in what way they could possibly be part of an explanation of linguistic data.

As I have shown in this section, Huffman's research about nasal flow levels only relate to specific manners and places of articulation and can only account for idiolects. Furthermore, her argumentation is circular and there is no evidence to support the claim that her proposals bear any relevance to linguistic research.

1.4.5 Brun, Spencer & Fourcin (1990): nasalisation detection using the electrolaryngography principle

In a paper entitled "Nasalisation detection using the electrolaryngography principle", Brun, Spencer & Fourcin ('BSF') [72] report about their ongoing research in relation to nasalisation dectection via an electrolaryngograph. While other researchers (*cf.* Netsell [339], Thompson & Hixon [474], Warren [487]) "have tried to measure air flow, air pressure and air speed from the nose and from the mouth" [72, p. 59], measurements obtained via an electrolaryngograph have in common with those established via an accelerometer (*cf.* Hoori & Monroe [224], Stevens, Kalikow & Willemain [457], Stevens *et al.* [458])[13] that they are derived through the nasal walls. In short, BSF find that

> "there is a small output from the nasal electrodes prior to and during the closure phase for the plosives /p/, /b/, /t/ and /d/" [72, p. 60].

In addition, their research

> "demonstrates clearly a significant output from the electrodes during nasalised vowels" [*ibid.*].

[13]BSF [72, p. 59] refer to "Stephens, 1975" and "Stephens, 1976". Given that the only similar entries in their bibliography are Stevens, Kalikow & Willemain (1975) [457] and Stevens, Nickerson, Boothroyd & Rollins (1976) [458], both of which are not referred to at all, I assume that Stephens (1975) refers to Stevens, Kalikow & Willemain [457] and Stephens *et al.* to Stevens [458].

Apparently, BSF's research is in line with other phoneticians' findings; in other words, phonetic nasality occurs in oral contexts, *i.e.* even during the closure phase of stops. However, even if BSF could establish a theoretical limit above which phonetic nasality does and below which it does not indicate phonological nasality, *e.g.* a Huffman-type orality threshold (section 1.4.4), their approach could still not be assumed to be phonologically relevant. The reason for this is that even though BSF's findings are only preliminary and are published in a paper which specialises on *work in progress*, the authors nevertheless manage to make it all too obvious that their research is based on an unrefutable version of the PH. The two quotes above demonstrate this clearly.

In the first quote, oral stop 'phonemes' are implicitly assumed to be phonologically oral, *i.e.* non-nasal; otherwise there would be no reason for BSF to establish when 'phonological' nasality (according to their views) actually corresponds to phonetic one while /p b t d/ would have to be declared phonologically nasal. It can thus be said that BSF's 'phonological' distinction nasality *versus* orality is made *a priori*, *i.e.* independently of the phonetically defined properties that are assumed to motivate phonology.

Also the second quote only makes sense if there are two kinds of phonological nasality for BSF, the type motivated by phonetics and the type which is assumed independently of phonetics. Remember that BSF found a significant output from the electrodes (indicating significant phonetic nasalisation) during *nasalised* vowels. The question I would like to raise here is: In what way have such nasalised vowels been established as nasalised ones? There are two answers in relation to our discussion of the phonological relevance of phonetic nasality: BSF must have motivated nasality in nasalised vowels either phonologically-phonetically or phonologically-non-phonetically. If the nasal status of the nasalised vowels in question were phonetically motivated, there would be no point to BSF's attempt to find out when phonetically nasalised vowels are phonetically nasalised, *i.e.* exhibit significant electrode output. This means that BSF (as virtually all phoneticians and phonetician-phonologists) assume that such 'phonologically' nasalised vowels can be established as such independently of phonetics, while at the same time phonology is assumed to be phonetically motivated because of which phonological 'nasality' is referred to as such in the first place. It seems that simplistic and unrealistic concepts like 'velum lowering' are good enough to establish phonological nasality, a notion which then has to be measured phonetically because the velum lowering hypothesis does not make accurate predictions. This also becomes obvious in BSF's section on "Further developments" [p. 61] within which they report about work which is undertaken to

> "study accurately the correlation between the laryngograph output from the nose and the predicted [*sic*] degree of nasalisation and occurence of nasals. The unmatched predictions will form a focus for further work, either to improve the prediction rules which do not take into account all what is known about nasalisation occurence, or to point out the limits of such a device" [*ibid.*].

As above, the 'predicted' degree of nasalisation is of course based on phonologically established nasalisation which, in this case is independent of phonetics—and the PH applies when it applies. Also note that 'unmatched predictions' will only cause BSF to improve the predictions rules or the device employed but not to challenge the PH and their entirely circular approach.

As always, the PH is set up in an unfalsifiable or almost unfalsifiable manner

Phonetic definitions of nasality: conclusion

Having looked at Entenman, Vaissière, Ladefoged, Huffman and BSF—and most of the references therein—I conclude that, to my knowledge, there is no phonetic definition of phonological 'nasality'. All mainstream phonological theories which assume the PH, *i.e.* all mainstream phonological theories, do so without evidence and are therefore irrelevant.

1.5 The Phonetic Hypothesis in action

In this section I will discuss Browman & Goldstein [69] (section 1.5.1), Kawasaki [248] (section 1.5.2), Hawkins & Stevens (1.5.3) and Clements & Hertz [104] (1.5.4) who incorporate a version of the PH into their frameworks. I will show that in each of these four cases, this results either in a number of wrong predictions which the respective authors simply ignore or a circular argument, also ignored. This will provide further evidence against a phonologically relevant and yet phonetically motivated definition of nasality.

1.5.1 Browman & Goldstein (1986): articulatory phonology

Browman & Goldstein ('BG') propose articulatory gestures as units which phonological representations are based on. Since movement is inherent in the definition of gestures they provide in BG's view "an explicit and direct description of articulatory movement in space and over time" [69, p. 222]. Speech in this framework is thus seen as a sequence of overlapping articulatory movements. According to BG, such a gestural analysis has the advantage over other phonological theories that it explains both articulatory movements and phonological structure and provides "a principled link between phonological and physical description" [p. 219].

BG provide two gestural analyses as evidence for their claim: an analysis of English sC-clusters[14] and a comparative analysis of English nasal-stop clusters and Chaga prenasalised stops. As I will show, both analyses make fundamentally wrong predictions about the phonological units involved.[15]

1.5.1.1 A gestural analysis of word-initial sC-clusters in English

In order to understand the behaviour of word-initial sC-clusters in English it is in BG's opinion important to understand the behaviour of the glottis in voiceless stops and clusters [69, p. 226]. BG show that only one glottal opening and closing gesture can be observed in sC-clusters; initial s *or* voiceless (aspirated) stops on their own demonstrate a similar gesture. To explain this, BG propose that words in English begin with maximally one glottal gesture. A further argument in favour of BG's "single-glottal-gesture generalisation" [p. 227] is that it correctly predicts the non-existence of a word-initial contrast between sb and sp: Since such a word-initial contrast would involve either a much smaller glottal gesture for sb than for sp or two gestures for sp, this contrast is precluded by BG's generalisation [*ibid.*].

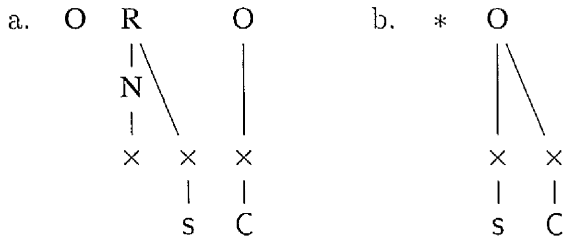
Let me now provide the evidence against BG's explanation. Kaye, Lowenstamm & Vergnaud ('KLV') [267] and Kaye [256] found that word-initial sC-sequences, *e.g.* in <u>sk</u>ip, as well as word-internal ones, *e.g.* in re<u>sc</u>ue, are never tautosyllabic but always heterosyllabic.

They argue that the distributional properties of and phonotactic constraints on such sequences in many Indo-European languages can be explained by assuming a structure as in (2a.) but not as in (2b.):

[14]BG only refer to sC-clusters where C is a stop ("/s/-stop clusters" [p. 226ff.]).

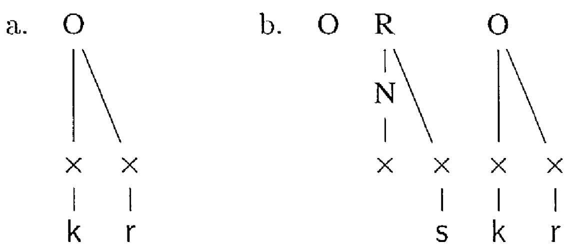
[15]For additional arguments against BG's gestural approach, *cf.* Clements [101].

(2)



Since word-initial sequences of 'stop plus liquid' (cream) in Indo-European languages have been shown to be associated to a constituent structure as in (3a.) (*cf.* [267]), sequences consisting of 's plus stop plus liquid' (scream) are predicted by Kaye [*ibid.*] to be linked to a structure as in (3b.):

(3)



In table 1.1 (p. 55) I provide some of the data Kaye and KLV use to motivate this analysis.[16]

The masculine definite article ('MDA') in Italian is il for all stems that start with a single consonant (*e.g.* s which is not part of a consonant cluster) or any well-formed consonant sequence other than sC (table 1.1a.–c.); sC-initial (d.–e.) and vowel-initial (f.) stems, on the other hand, have lo.[17] This shows that initial s on its own behaves like any other filled non-branching or branching onset. In opposition to that, sC-

[16]KLV refer to Nespor & Vogel [338] and Chierchia [94] for a discussion of *raddoppiamento sintattico* [267, p. 227, footnote 11]. Kaye has the medial or passive (perfect) form πε-πεπαίδευμαι (pe-paideumai), not the active form given in table 1.1; the Ancient Greek examples sesôka and estugêka are not provided by Kaye or KLV.

The following key has been used for the transliteration of the Ancient Greek data—Kaye uses Greek letters. Note that only those letters which occur in this section are given. Accents have been neglected:

α	a	ϵ	e	θ	tʰ	λ	l	ξ	x	σ	s	ϕ	pʰ
ω	ô												
γ	g	ζ	z	ι	i	μ	m	π	p	τ	t	χ	kʰ
δ	d	η	ê	κ	k	ν	n	ρ	r	υ	u	ψ	ps

[17]The fact that vowel-initial stems drop the o in lo is accounted for by the Obligatory Contour Principle (*cf.* Leben [291]). Note also that for masculine nouns starting with z the MDA is il in some varieties and lo in others. When it is lo, z appears to be linked to a postnuclear rhymal position and simultaneously to the following onset, *i.e.* the same structure s and a following consonant in an sC-cluster are attached to (*cf.* 2a.). Since in such cases z is linked to two skeletal points, it is realised as so-called 'geminate' or 'long' consonant.

clusters pattern with vowel-initial stems. It follows that BG's analysis, which groups the data in (table 1.1a.–e.) together, makes wrong predictions about the phonological behaviour of sC-sequences and is therefore irrelevant. A more relevant phonological explanation is that il is selected for stems beginning with onsets which dominate a skeletal point, while lo is used for stems starting with onsets which dominate no point.

The *raddoppiamento sintattico* data illustrates that in Italian word-initial onset heads geminate when preceded by a word-final stressed vowel. As in the case of the MDA, s on its own patterns with other onsets. However, sC-clusters do not: *[kafésspésso] or *[tʃittásstraniéra] are ill-formed. This provides further evidence in favour of KLV's and Kaye's heterosyllabic analysis of sC-sequences.

In Ancient Greek, one of the steps necessary to form the perfect stem of a verb is to affix Ce- to a consonant-initial verb stem, where C is a copy of the stem-initial onset-head, unless the verb stem starts with an sC-cluster (or r, x, ps or z). sC-initial stems prefix e-, not *Ce-, and, as in Italian, do not behave like other stems beginning with a filled onset. Consequently, BG's analysis of English has to disregard phonological evidence from both of these languages.[18]

In European Portuguese, the negative prefix in- only occurs in this form when affixed to a stem beginning with an onset that does not dominate a skeletal point (table 1.1d.–f.). If the stem-initial onset does dominate a point (a.–c.), the negative prefix changes to ĩ-. However, an application of BG's analysis of English s-stop sequences to European Portuguese predicts word-initial sC-clusters to pattern with other consonants or consonant sequences. As in Italian and Ancient Greek, this cannot be observed in European Portuguese.

This means that even if there were no phonological data from English against BG's gesture analysis, BG's account of English sC-sequences would have to ignore cross-linguistic evidence from Italian (MDA plus *raddoppiamento sintattico*), Ancient Greek and European Portuguese. BG's approach would therefore have to see Universal Grammar as a concept which does not necessarily always apply to En-

[18]There is no space here to discuss what probable phonetic realisations of r, x, ps or z were. The relevant forms show that they were probably heterosyllabic sequences. Like sC-initial stems but unlike other consonant-initial stems they form the perfect stem by affixing the prefix e-: errip^{h}a 'I have thrown' (riptô 'I throw'); e-xenôka 'I have taken in as guest(s)'; e-pseusa 'I have tricked'; e-zêlôka 'I have envied'. There is also no space to look in more detail at the observable aspiration dissimilation (Grassmann's Law in Sanskrit and here in Greek, Dahl's Law in Bantu languages; for my analysis of Dahl's Law in Kikuyu and Augsburg Swabian, *cf.* chapter 5): ke-k^{h}ôrêka 'I have gone away'; pe-p^{h}ilêka 'I have loved'; te-t^{h}uka 'I have sacrificed'. Neglecting Grassmann's Law, k^{h}, p^{h}, t^{h} clearly pattern with stems beginning with a filled onset and not with sC-clusters.

glish. This, in turn, would be tantamount to declaring English an extraterrestrial language. Consequently, BG's phonological account of English sC-clusters turns out to be highly dubious independently of whether or not their gesture-based theory works for English. Unfortunately though, such language-specific exceptions to phonetically motivated or grounded predictions are completely non-controversial in frameworks which incorporate the PH. For example, if the palatalisation of t or s in Japanese preceding i resulting in ʧ or ʃ (respectively) is accounted for by pointing to an explanation which makes reference to musculatory effort, it is necessary to claim that speakers of other languages (*e.g.* of German) can simply do without this type of musculatory ease. Of course, supporters of such an 'explanation' would not be able to make any predictions about how frequently such articulatory forces play a role or, alternatively, have no effect at all (*cf.* AP's grounding conditions, (1) in section 1.3.2).

Additionally, there *is* evidence from English against BG's analysis. As Kaye [pp. 300ff.] demonstrates, English varieties show varying constraints on the occurrence of postconsonantal j. In New York English, tonic and pretonic postconsonantal j is lost after coronals, *e.g.* tune (*t[j]une), while many Southern British varieties allow j after all coronals except r, and, in some varieties, also except l, *e.g.* t[j]une (*tune) and rude (*r[j]ude) for all varieties, l[j]urid dialectically alternating with lurid. Those speakers of British English who keep [j] after l (lj-speakers) can never have [j] after branching onsets, *i.e.* after sequences like pl, bl, kl. So while these speakers say l[j]urid and all[j]ure, they have pluvial (*pl[j]uvial), blue (*bl[j]ue), clue (*cl[j]ue). As mentioned above, in BG's gesture analysis all onset types are characterised by one glottal gesture.[19] Why for lj-speakers [j] is ill-formed after consonant sequences like pl, bl, kl but well-formed after sC-sequences (stupid (st[j]upid), spew (sp[j]ew) and skewer (sk[j]ewer)) remains unclear in a gestural approach. It follows that BG's phonetically based analysis is not only in conflict with evidence from languages other than English which display sC-clusters: it can also not be backed up by any English facts and, due to the identical treatment of sC-clusters as opposed to other clusters, makes wrong predictions about English. Even though BG's observations might be phonetically relevant, *e.g.* for speech implementation, there is no independent phonological evidence for them. This means that there is also no evidence for the assumption that gestural movements play any part whatsoever in the phonology of English sC-clusters or those of any other language. It is

[19]The only criterion BG's analysis is dependent on is manner of articulation. While the peak glottal opening occurs with the midpoint of a fricative, it coincides with the release of a stop gesture [69, p. 228].

now clear that one of two analyses BG provide as arguments in favour of their claims does not contain any phonological insights.

1.5.1.2 Chaga prenasalised stops and English nasal-stop clusters

BG [pp. 229–237] compare Chaga prenasalised stops with English nasal-stop clusters[20] and try to show that a phonological framework based on articulatory gestures can adequately explain the distributional patterns of such sequences. Based on Anderson [9], BG [p. 229] provide the following feature matrices to distinguish prenasalised stops (4a.) from nasal-stop clusters (4b.):

(4)

<table>
<tr><td></td><td>a.</td><td>m</td><td>b</td><td>b.</td><td>m</td><td>b</td></tr>
<tr><td>cons</td><td></td><td colspan="2">+</td><td></td><td>+</td><td>+</td></tr>
<tr><td>nasal</td><td></td><td>+</td><td>−</td><td></td><td>+</td><td>−</td></tr>
<tr><td>ant</td><td></td><td colspan="2">+</td><td></td><td>+</td><td>+</td></tr>
<tr><td>cor</td><td></td><td colspan="2">−</td><td></td><td>−</td><td>−</td></tr>
<tr><td>⋮</td><td></td><td></td><td></td><td></td><td></td><td></td></tr>
</table>

In BG's view, "the structures represented in [(4a.) and (4b.)] might be expected to lead to different phonetic entities" [*ibid.*]. In terms of articulatory gestures, one would predict a gestural differentiation between the bilabial closure gestures of prenasalised stops as opposed to nasal-stop clusters. BG would expect a single bilabial closure gesture ('BCG', abbreviation mine) for prenasalised stops and two BCGs or one longer BCG for nasal-stop clusters.

If English nasal-stop clusters as in ca<u>mp</u>er and ca<u>nk</u>er are analysed as segment sequences, a gestural analysis could only back this up if the sequences in question were to display either two BCGs or one BCG longer than a BCG in a prenasalised stop. However, in English, BG find a single BCG "regardless of whether the consonantal portion is described as a single consonant (/b/, /p/ or /m/) or as a consonant cluster (/mp/ or /mb/)" [p. 233]. Similarly, in Chaga there is only one BCG for single and for prenasalised voiced consonants (p, m, mb). Chaga mp, on the other hand, shows in BG's opinion the result of two overlapping BCGs. To illustrate their point, BG provide the gestural symbols in (5) [p. 241] and the gestural constellations in

[20]Chaga (kiChaka) is a Bantu language spoken in Tanzania, BG's Chaga data is taken from Nurse [345]. For their research BG recorded a male speaker of Chaga and a female speaker of American English [p. 230].

figure 1.1 [p. 244]:[21]

(5)

Symbol	*Gesture*
β	bilabial closing and opening
γ	glottal opening and closing (returns to voicing position)
$+\mu$	velic opening (nasal)
$-\mu$	velic closing (oral)
V	vowel

Figure 1.1: Browman & Goldstein's gestural matrices for English nasal-stop sequences and Chaga prenasalised stops [69]

a. English

cammer	cabber	capper	camber	camper
V V	V V	V V	V V	V V
β	β	β	β	β
		γ		γ
$+\mu$ $-\mu$	$-\mu$	$-\mu$	$+\mu$ $-\mu$	$+\mu$ $-\mu$

b. Chaga

mpaka
β V
β
γ
$+\mu$ $-\mu$

If gestural representations as in figure 1.1 are assumed to motivate phonology, English nasal-stop clusters and Chaga single stops, nasals and prenasalised voiced stops are, in my opinion, predicted by BG to occur as phonological class, *i.e.* to display similar patterns of phonological behaviour; mp (with syllabic m), on the other hand, should behave differently. Unfortunately, BG do not discuss this prediction.

Let us now try to find out whether BG's gesture-based account of nasal-stop sequences and prenasalised stops can help us to understand the behaviour of the phono-

[21] The gesture specifications for β are based on articulatory measurements made by BG; all other specifications are estimates by BG [pp. 241f.]. Chaga words beginning with m, p or mb "have representations like those of the comparable English words, except that the initial V is not present for these words" [p. 243]. Note also that Chaga mpaka has syllabic m [m̩paka] [p. 243]. BG differentiate Chaga [m̩p] from English [mp] by assigning the relevant Chaga sequence a second BCG. This BCG is represented on the top line, *i.e.* the vowel line, in order to account for the syllabic nature of its nasal stop [p. 245].

logical units involved. Opposed to English nasal-stop clusters, Chaga prenasalised stops occur word-initially. BG [p. 236] try to explain this difference in the following way:

> "The simplest statement is as a distributional, or phonotactic, difference. That is, in Chaga, such gestural structures can occur in word (and/or syllable) initial position, whereas in English the same gestural structures cannot occur in initial position."

As it turns out, BG's explanation of the phonotactic difference in question consists merely of a description of this difference. BG's account itself makes no predictions about the phonological behaviour of nasal-stop sequences or prenasalised stops that can be supported by independent evidence, which, astonishingly, is not in conflict with BG's scientific methodology.[22] I can therefore claim that BG's analysis does not add anything to an explanation of the attested distributional difference. As in the case of sC-clusters, the PH fails.

Articulatory phonology: conclusion

It is now obvious that BG's gestural framework makes no phonologically relevant predictions about any of the data provided by them. Articulatory phonology as presented by BG has no explanatory power and is totally ineffectual as phonological framework. How BG can possibly come to the conclusion that "such gestural descriptions are useful as a basis for phonological description" [p. 240] remains highly unclear.

1.5.2 Kawasaki (1986): experimental phonology

Kawasaki follows Hyman's [234] hypothesis that cases of denasalisation of nasal stops preceding OVs in languages with nasal-oral vowel opposition adjacent to nasal and oral consonants is due to perceptual reasons. Let me first provide some examples:[23]

[22]Note that BG do not admit that, in spite of their precise phonetic measurements, their approach is not empirical nor do they discuss in what way their findings provide new insights. It seems that BG adhere to the self-delusional method.

[23]Kawasaki [p. 85] states that "nasal consonants are realized as partially denasalized near OVs in Amahuaca [357], Apinaye [73], Gbeya [427], Guarani [187], Otomi [50], Siriono [398], and Wukari Jukun [492, 234]". The references within square brackets in this quote are taken from Kawasaki [248, pp. 96ff.]. The examples in (6) are from Gregores & Suárez [187] for Guaraní and from Salser [425]

(6)

Guaraní		*Cubeo*	
mẽ^{n}daré	'widow(er)'	^{n}dahákɨ	'come!'
mẽ́nã	'spouse'	nãh̃ṍkó	'shrimp'
^{m}baʔé	'thing'	^{m}bákó	'mama'
mãʔẽ́	'seeing'	ɲãmã́ko	'deer'

In both Guaraní and Cubeo, NCs are realised as (prenasalised) voiced stops when followed by an OV.[24] In Kawasaki's view,

> "some kind of automatic or commonly encountered perturbation of one segment by another [*i.e.* nasalisation of vowels after nasal stops] may be taken for granted and factored out of the phonetic percept constructed for a word, as long as the segment responsible for the perturbation is detected [...] If the perturbing segment is not detected, then the perturbation is not expected and is not factored out; it is then included as part of the phonetic percept of the word" [pp. 86f.].

Kawasaki supports her claim by two experiments with native American English speakers which show that "the degree of perceived nasality of a vowel is enhanced by the attenuation of adjacent nasal consonants or, conversely, is reduced by the presence of adjacent nasal consonants" [p. 94]. In languages like Guaraní and Cubeo a nasalised vowel preceded by a nasal stop is a perceptually ambiguous situation: an underlyingly oral vowel following a nasal stop is phonetically, *i.e.* non-distinctively, nasalised; a nasalised vowel following a NC could thus be underlyingly oral or nasal. Kawasaki's proposal is that as long as the source of a nasalisation process, the nasal stop, can be detected (perceptually) the nasalisation in the target, the following nasalised vowel, can be weak. However, if the source is undetectable, nasalisation in the target is strong. In Guaraní and Cubeo with underlying nasal-oral distinction in vowels, nasalisation of vowels following nasal stops is predicted to be weak because the source, *i.e.* the nasal stops, are clearly nasal. On the other hand, NCs are denasalised or, in other words, made more undetectable (as regards nasality) when preceding underlyingly oral vowels. This results in a situation where such OVs will

for Cubeo. Kawasaki does not mention Cubeo. For Sirionó, *cf.* also Firestone [164]; for Guaraní, Lunt [310] and Rivas [412]. Acute accents symbolise stress in Guaraní but high pitch in Cubeo.

[24]In these languages there is a phenomenon that I would like to refer to as nasal sharing: there are no voiced stops preceding NVs and no nasal stops preceding OVs. For example, *^{m}bã, *ma are ill-formed, while ^{m}ba, mã are well-formed.

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be perceived clearly oral since NVs in the same context would be strongly nasalised due to the low degree of detectability of denasalised nasal stops. Consequently, so Kawasaki, denasalisation of this type can be explained as perceptually and ultimately phonetically motivated phenomenon.

The main problem of Kawasaki's explanation of alternations between nasal and (prenasalised) voiced stops is that it ignores all cases where languages have nasal-oral vowel oppositions in the context of nasal and oral consonants (just like Guaraní and Cubeo) but where nasal stops do *not* get denasalised when preceding OVs. Consider the following examples from French and Auca (Pike & Saint [375]), an Ecuadorian language:[25]

(7)

French			*Auca*	
$^{(\mathrm{m})}$bãkɛ́	⟨banquet⟩	'banquet'	$^{(\mathrm{m})}$bã́mõ	'whole seed'
$^{(\mathrm{m})}$bakɛ́	⟨baquet⟩	'bucket, tub'	$^{(\mathrm{m})}$békã	'he drinks'
mãké	⟨manqué⟩	'missed'	mã́mõ	'take, bring'
makí	⟨maquis⟩	'thicket resistance group'	ṍma	'feather'

The data in (7) provide clear counterexamples to Kawasaki's hypothesis. In French and Auca, nasal stops are *not* denasalised before OVs (French makí, Auca ṍma). It remains an open question under which circumstances Kawasaki's hypothesis will work and when it will not. If NCs get denasalised when preceding OVs in order to increase the perceived degree of orality of these OVs, why can French and Auca speakers handle the hypothesised perceptual difficulties such sequences entail, without denasalisation of the nasal stops? Even if there were some evidence for the claim that NCs in present day French undergo denasalisation when preceding OVs, the assumption that this can be motivated phonetically leaves it unclear why the phonetic result of such a process, *i.e.* denasalised nasal stops, is different from the result in *e.g.* Guaraní. In other words, this version of the PH says nothing about why nasal consonants (presumably denasalised) are realised as prenasalised voiced stops in Guaraní while they are not in French. Furthermore, whenever one were to look at a language which has a 'surface' series of nasal and voiced stops and of nasal and oral vowels

[25] Acute accents in French transcribe stress; in Auca they represent high pitch. However, Pike & Saint [*ibid.*] and Pike [374] refer to this suprasegmental phenomenon as "stress". Note that domains ('words') in Auca can be 'stressed' on more than one vowel. Pike [374] even discusses "stress trains" and "wave trains". The French data is taken from my notes.

and were to ask whether Kawasaki's hypothesis will work in this language, the best, *i.e.* most precise, answer one could give is 'maybe'.

Finally, note also that in Kawasaki's view, listeners construct *phonetic percepts* for words (*cf.* above quote). The notion 'phonetic percept' interestingly already entails an abstraction which can, to my knowledge, not be motivated phonetically by any linguist or phonetician. We see again that the phonology is not motivated phonetically. Kawasaki's explanation simply revolves around the fixed idea called 'PH'.

1.5.3 Hawkins & Stevens (1985): acoustic correlates

A similar perceptually motivated approach can be found in Hawkins & Stevens [209] who make the following statements:

> "Some languages have the same number of nasal as non-nasal vowels, with no reported differences in quality between the two sets. In a substantial minority of languages that contrast nasal and non-nasal vowels, there is a reduced number of nasal vowels ... Most commonly it is the mid vowels that are missing in these imbalanced systems ... The problem of reduced discriminability [caused by nasalisation] is thereby avoided in that only those vowels with the most distinctive values of $F1$ are retained" [209, p. 1574].

Similarly to other phonetically motivated explanations, Hawkins & Stevens account for a phonological pattern, *e.g.* missing nasalised mid vowels, phonetically, *i.e.* as avoidance of nasality-induced reduced discriminability. It does, however, not seem to matter to them that, contrary to their predictions, many languages do not display any difficulties with the phonetically motivated 'phonological' system that some languages, in line with their assumptions, try to avoid. So if a number of languages do not exhibit nasalised mid vowels due to perceptual reasons, why do such vowels occur in other languages (without any evidence that the nasal mid vowels which do occur are necessarily unstable)? Like Kawasaki's approach, Hawkins & Stevens's explanation apparently works when it works.

It is now clear that Kawasaki's (and Hawkins & Stevens's) acoustically or perceptually motivated proposals add nothing to an explanation of the phenomena involved.

1.5.4 Clements & Hertz (1996): an integrated approach to phonology and phonetics

Another perceptually oriented approach can be found in Clements & Hertz [104]. They propose that

> "given an appropriately integrated framework for phonological and phonetic description, the optimal representations required for the expression of generalizations at both levels, phonological and phonetic, will be largeley *congruent* [their emphasis]: that is, we should find no substantial mismatch between surface-phonological representation and phonetic representations, the latter consisting largely of a fuller specification of the former" [p. 144].

The question which arises here is: What kind of mismatch would be substantial enough for Clements & Hertz to drop their version of the PH? Since they do not discuss this, it becomes apparent that they are not interested in falsification of their view. How this in turn can still be seen as empirical approach is unclear. The unfalsifiability typical for such 'integrated' approaches also becomes evident in Clements & Hertz's conclusion that the "phonetics can be viewed as forming a grammatical system" [p. 169] and that this "phonetic component of grammar contains both universal and language-specific [*sic*] principles" [p. 170]. I have already discussed the untestable status of so-called phonetic motivation that only shows itself in a language-specific manner (*cf.* strategy 2, section 1.3.2). Note though that, as Kaye [253] points out (*cf.* section 1.2), phonetic processes *do* occur but that they are universal and omnipresent and cannot be used to distinguish one (language-specific) linguistic system from the other. Since Clements & Hertz's theoretical framework is based on underspecification (*cf.* my discussion in section 2.3)[26] it is not surprising that their version of this theory is circular too.

Let me provide one example. As part of their Integrated Representational System, Clements & Hertz subcategorise the spectogram of General American English[27] ⟨tide⟩ [tʰajd][28] into phones and transitions. Phones are "the portions of the sig-

[26]Clements & Hertz refer to the findings of Browman & Goldstein [69, 70], Clements [102], Clements & Hume [105], Cohn [114], Hertz [214, 215, 216], Keating [268], Kenstowicz [271], Pierrehumbert & Beckman [367]. Additional literature, *cf.* Archangeli [14, 15], Browman & Goldstein [71], Clements & Hertz [103, 104], Pierrehumbert [366].

[27]There is no evidence for the existence of 'General American English' or any language concept referring to a linguistic system that underlies numerous mutually intelligible 'varieties' of that system. Languages are not related; *cf.* section 3.2.3.

[28]Clements & Hertz use 'y' for 'j'.

nal that correspond to the time intervals during which the lips and tongue maintain the target positions appropriate for the production of each phonological root node" [p. 146]; transitions are the portions between the phones of an acoustic string. More precisely, "any root node characterized by at least one formant target is termed a *phone*" [p. 149]. Clements & Hertz provide the following "multitiered representation" [*ibid.*]:

(8) *Multitiered representation of F2 and duration values for English* ⟨*tide*⟩

skeleton :	×		×		×		×
root tier :	t		a		j		d
duration tier (in ms) :	85	115	85	75	20	10	65
F2 (in Hz) :	2100		1350		2150		2040

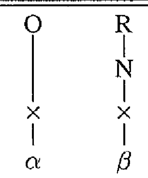
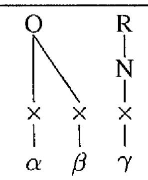
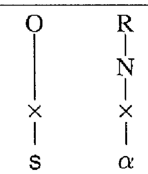
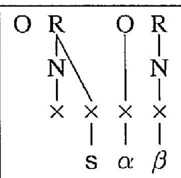
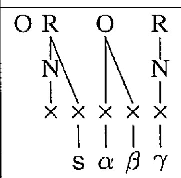
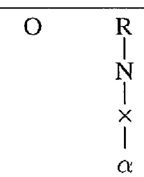
In other words, Clements & Hertz do not motivate phonetically which parts of a given acoustic string are phonologically relevant. Since the phones in (8) are t, a, j and d, it appears that the stable (non-transitional) chunks of an acoustic string are motivated by what is considered a relevant portion of the root tier. The root tier, however, is motivated cognitively ('phonologically'): Clements & Hertz cannot investigate an acoustic string phonetically and decide on the basis of any of its phonetically defined properties which of its sections corresponds to a root node. Neither the placement of root nodes in a string nor the decision which subset of the set of phonetically circumscribable phenomena is surface-phonologically relevant ([t a j d]) is predictable from phonetic measurements. This always has to be motivated by an analysis of the system of contrasts (segment inventory) operative in a language, which is not predictable from phonetic measurements. This means that the relevant root nodes are, in fact, not phonetically motivated but are merely claimed to be.

Based on this dubious claim, a supposedly phonetically motivated string then has to be investigated phonetically to find out in what way presumed phonetic motivation actually corresponds to measurable phonetic properties. As in the case of Browman & Goldstein—or, in relation to diachronic linguistics, Dolbey & Hansson (*cf.* section 1.3.3)—, the fact that numerous phonological phenomena are phonetically natural misleads these researches into thinking that this link between phonology and phonetics consists of a causal relationship from the phonetics to the phonology (and back and forth, *etc.*). Without an independent criterion on the basis of which it could be decided when the PH may not be applied, few phonetically non-natural phonological phenomena and no language in which a process or phonotactic constraint does not occur could ever provide evidence against the PH.

Conclusion

I have shown in this chapter that there is a vast amount of evidence available against any articulatory or acoustic version of the PH. Furthermore—and as I will discuss in more detail in chapter 2—, there is to my knowledge no phonological framework that supports the PH and that does not have to either ignore the vast number of counterexamples or simply allow for them.

Table 1.1: sC-clusters in Italian, Ancient Greek and European Portuguese

	Stem-initial 'syllable(s)'	*Italian MDA*	*Raddoppiamento sintattico*	*Ancient Greek perfect active*	*European Portuguese* in-
a.		il costo 'the price'	paltó pulíto [paltóppulíto] 'clean coat'	pe-paideuka 'I have educated'	[ĩ]capaz 'incapable'
b.		il treno 'the train'	città tríste [tʃittáttríste] 'sad city'	ge-grap^{h}a 'I have written'	[ĩ]tratável 'unsociable'
c.		il sale 'the salt'	città sánta [tʃittássánta] 'holy city'	se-sôka 'I have spared'	[ĩ]satisfeito 'dissatisfied'
d.		lo slancio 'the élan'	caffè spésso [kaféspésso] 'thick coffee'	_e-stugêka 'I hate'	in[ʃt]imável 'inestimable'
e.		lo sprezzo 'the scorn'	città straniéra [tʃittástraniéra] 'foreign city'	_e-strateuka 'I have taken the field'	in[ʃkr]upuloso 'unscrupulous'
f.		l'arco 'the arch'			inobediente 'disobedient'

Chapter 2

The Feature [Nasal]

Introduction

This chapter argues against the phonological relevance of the commonly employed feature [nasal].[1] Since [nasal] is on the one hand based on *phonetics* and on the other hand a *feature*, I have provided evidence against a phonologically relevant definition of the supposedly phonetically defined concept 'nasality' in chapter 1. Following this, I will in this chapter look at various feature theories which make the PH part of their basic set of assumptions.

In short, what I would like to show in this chapter is that the notion 'feature'—if it refers to phonetically defined or non-monovalent systems—is not a phonologically relevant one and that any phonological theory that employs such features is fundamentally flawed. I will start this discussion by showing that feature theories without feature co-occurrence constraints always generate more phonological expressions than can be observed world-wide (section 2.1). Subsequently, I will look at a number of mainstream techniques employed by modern phonologists to avoid this kind of overgeneration. I will start by showing that neither language-specific monovalency (2.2.1) nor redundancy rules (2.2.2) are able to provide a relevant explanation of phonological phenomena (2.2). Since all of these 'techniques' are currently employed by Underspecification Theory, I will also argue against Keating's [268] and Pulleyblank's [403] proposals about phonetic and phonological underspecification and provide evidence for my claim that the PH contained in underspecification is not a scientific assumption (2.3). At the end of this chapter, after a brief discussion of phonetically motivated monovalent features (2.4), I will look at the short-comings of feature geometry (2.5) first by demonstrating in what way the motivation for feature

[1] An ealier version of this chapter can be found in Ploch [382].

geometry is not an empirical one (2.5.1) and then by exemplifying these findings via a discussion of the problems evident in Piggott's [369] feature geometry-based analysis of nasal harmony phenomena (2.5.2).

2.1 Overgeneration

Categorising features according to valency, three types of features can be found: single-valued (monovalent), binary and scalar (or multivalent) features. The main disadvantage of non-monovalent systems is that the number of features necessary to generate all universally observable phonological expressions inevitably leads to overgeneration. Consider the following argument:

Firstly, there is to my knowledge no non-monovalent feature theory which does not have to stipulate at least 10 features.

Secondly, the number of phonological expressions necessary to account for all phonological phenomena world-wide occurring on all assumed levels of representation is limited: In most phonological frameworks there are at least two levels of representation, the underlying level (the lexical level (LL) in GP, *cf.* Kaye [259]) and at least one derivational level (GP has one, the phonological level (PL)). Counting all of the phonetic symbols currently employed by phonologists on all stipulated levels, *e.g.* all of the phonetic symbols in the IPA chart (revised to 1993, *cf.* Nolan [343]) and even including expressions generated by diacritics, *e.g.* dental [n̪] (as opposed to 'merely' alveolar [n]), it is difficult to reach a number higher than, let us say, 512.[2] Thirdly, neglecting language-specific co-occurrence constraints and excluding the co-occurrence of more than one feature value within a phonological expression universally, all value-settings of all assumed features should be allowed to merge freely. Since a theory with only binary features generates 2^n phonological expressions (PEs) —where n stands for the number of features in use—a system with 1 binary feature generates 2 PEs, one with 5 features, 32 PEs, and so forth (*cf.* table 2.1 [p. 58]).

Consequently, a theory with more than 9 binary features with no co-occurrence restrictions always generates a number of PEs which is much higher than the number of PEs necessary for phonological analyses.[3]

[2] Ladefoged [283, p. 16] provides an estimate of 600 to 800 contrasting segments which, in his opinion, is backed up by a "comparable number" in Maddieson [312]. Whether the assumed number is closer to 512 (as above) or 600 to 800 is irrelevant here.

[3] I consider this to be a conservative estimate; there is in my opinion no evidence for the phonological relevance of more than around 100 to 120 PEs.

Table 2.1: Number of phonological expressions predicted by binary features

Number of binary features in use	*Number of predicted PEs*
1	2
2	4
3	8
4	16
5	32
6	64
7	128
8	256
9	512
10	1,024
15	32,768
20	1,048,576

2.2 Counteracting overgeneration

To avoid this kind of overgeneration, most current theories employ mainly three types of co-occurrence constraints—if they are, unlike Optimality Theory, interested in internal representations at all: language-specific monovalency, redundancy rules and feature geometry. In this section I will look at the first two types. Feature geometry is discussed in 2.5.

2.2.1 Language-specific monovalency

The fundamental problem of the proposal of language-specific monovalency is that while the identification of features is assumed to be motivated by properties of the articulatory system, observable cross-linguistic tendencies for a certain value of a feature must remain completely unmotivated by these properties. Even if there is a strong tendency for a given feature to have a certain value-setting and if this tendency is captured by a theory by the proposal of markedness conventions based on phonetics, all cases where a language-specific setting is contrary to the observed tendency are counterexamples and must be allowed for by mere stipulations (*cf.* section 1.3 regarding AP's 'Grounding Conditions'). So language-specific monovalency of articulatory features is an *ad hoc* way of accounting for the numerous counterexamples to the PH.

2.2.2 Redundancy rules

Similarly, there are no proposed redundancy rules known to me which can provide an explanation of the observable redundancies that is based on independent evidence. All redundancy rules are either completely *ad hoc*, unfalsifiable or describe tendencies which may be violated in an unspecified number of languages via yet another type of redundancy rule. Let me provide two examples: AP [17] and Steriade [454].

2.2.2.1 Archangeli & Pulleyblank (1994): Default and Complement rules

AP propose two types of redundancy rules, Default and Complement rules. Default rules formalise universal co-occurrence constraints like *If [+low] then [−high]* and, as Steriade [p. 342] puts it, "introduce what are considered to be universally unmarked feature values such as [+high] for [high]".[4] Complement rules, on the other hand, account for the language-specific value-setting of a feature if no Default rule is able to predict it. If we apply this differentiation between Default and Complement rules to the discussion of AP's Grounding Conditions above, the general incoherence of AP's approach becomes evident: If content and nature of the Default rules proposed is motivated by or 'grounded' in articulatory phonetics, all cases where AP have to propose a Complement rule are examples of phenomena predicted to be non-existent by the assumed Default rules. In other words, every Complement rule proposed is an overt stipulation whose one and only task is to give counterexamples to assumed Default rules a label. Surprisingly, it does not appear to be relevant to AP that they create a system which is only loosely linked to the scientific concept of falsifiability. This attitude exemplifies the difference between a cognitively and a phonetically motivated view of phonology: Since the motivation of the phonetic approach contains a (non-testable) phonetic part and one independent of phonetics (*i.e.* a cognitive part) while the cognitive approach does not subscribe to the phonetic part, the phonetic approach always exhibits a lower degree of testability than the cognitive approach.

[4] Steriade [454] does of course not refer to AP [17]; she discusses the distinction between Default and Complement rules as proposed in Archangeli [14] and AP [16]. However, there has been no theoretical change relevant here between Archangeli's or Pulleyblank's earlier work quoted above and AP [17].

2.2.2.2 Steriade (1987): Redundant and Distinctive values

Steriade [454] proposes a different kind of distinction: for her, redundancy rules introduce either "Redundant values (R-values)" or "Distinctive values (D-values)" [pp. 341ff.] independently of whether they apply universally or language-specifically. Consider the redundancy rules in (1) [p. 341] when applied to the system in (2) [*ibid.*]:

(1)

a. [+sonorant] $\longrightarrow$ [+voice]

b. [ ] $\longrightarrow$ [−voice]

(2)

	p	t	k	s	b	d	g	z	m	n	l	r
son	−	−	−	−	−	−	−	−	+	+	+	+
cont	−	−	−	+	−	−	−	+	−	−	−	+
voic					+	+	+	+				

According to Steriade [*ibid.*], the rule in (1a.) "introduces a non-underlying specification [+voice] within a segmental class, that of sonorants, where the feature is predictable", *i.e.* an R-value. The rule in (1b.), on the other hand, "introduces the non-underlying feature value within a segmental class, that of obstruents, where the [*sic*] both of its values occur freely" [*ibid.*], *i.e.* a D-value. Steriade sees the main advantage of this distinction in that it correlates with observable phonological phenomena and avoids the AP-type categorisation of redundancy rules according to whether they express feature values predictable from universal or, alternatively, language-specific facts (Default *versus* Complement rules, respectively). This, so Steriade [p. 358], is a welcome effect since the distinction proposed by AP "appears to play no role in the functioning of phonological systems". I will provide one of Steriade's examples here to illustrate her point: liquid dissimilation in Latin. Let me sum up Steriade's data [p. 351]:[5]

[5]Steriade's Latin data is presented within phoneme (or morpheme) slashes without length distinctions, *e.g.* "/nav-alis/" instead of navaːlis. I assume the Latin letter ⟨c⟩ to be k. Note that in traditional and/or historical grammars Latin nouns in either -us (nominative singular masculine), -iː (genitive singular masculine) or those in -um (nominative singular neutral), -iː (genitive singular neutral) are considered to be 'o-stems; such o-stems are lati-(o-), retikul-(o-), sepulkr-(o-). However, there is to my knowledge no evidence which supports the proposal of o-stems within a synchronic analysis of Latin because of which I will not refer to them as such.

(3) *Latin liquid dissimilation*

a. *Stem*	b. *Stem* + -aːris ~ -aːlis	c. *Gloss of* b.
naːv-	naːvaːlis	'naval'
soːl-	soːlaːris	'solar'
miːlit-	miːlitaːris	'military'
lati-	latiaːris	'of Latium'
retikul-	retikulaːris	'of the net'
floːr-	floːraːlis	'floral'
sepulkr-	sepulkraːlis	'funereal'
liːtor-	liːtoraːlis	'of the shore'

The suffix /-alis/ (/nav-alis/) turns into /-aris/ when following a stem containing /l/ (/milit-aris/). However, if /r/ intervenes between /l/ and /-alis/, dissimilation fails to take place (/flor-alis/).[6] According to Steriade's analysis, the observable dissimilation phenomenon occurs under strict adjacency at the relevant tier, *i.e.* the tier of [lateral] specifications. All non-liquids are redundant non-laterals (R-value) and are therefore unspecified for [lateral]. This means that all non-laterals except /r/ are unspecified for [lateral] (D-value). Let me provide two illustrations. The example in (4b.) is taken from Steriade [p. 351]:

(4)

a.		[+lat]					[−lat]	
		|					|	
	mi	l	i	t	–	a	r	is
b.		[+lat]		[−lat]			[+lat]	
		|		|			|	
		l	ito	r	–	a	l	is

In (4a.), the /l/ in /-alis/, specified as [+lat] dissimilates to /r/ ([−lat]); the intervening consonant /t/ does not block the dissimilation process because it is unspecified for laterality and is thus not projected to the tier of lateral specifications. The example in (4b.) shows that within this framework /r/ must be specified as [−lat] and consequently be projected to the lateral tier because it blocks the dissimilation that would otherwise be triggered by two adjacent segments specified as [+lat]. The

[6] Steriade [*ibid.*] does not mention the alternative form latiaːlis (*cf.* (5) and Lewis & Short [293]).

important point for Steriade is that non-liquids are non-lateral via a redundant, *i.e.* non-distinctive, assignment for feature values while /r/ is non-lateral via a distinctive assignment. In other words, Latin liquid dissimilation appears to provide evidence for the proposal of the phonologically relevant distinction of R- and D-values. However, there seems to be no evidence for AP's differentiation between universal and language-specific redundancy. In Steriade's view [p. 358], "the language specific non-laterality of obstruents is as irrelevant in Latin as the universal non-laterality of nasals and vowels".

Neglecting independent arguments against Steriade's proposal, I agree with her opinion that AP's assumption of Default and Complement rules does not appear to be phonologically relevant. However, Steriade's argumentation for the distinction of R- and D-values has a number of fundamental problems: Firstly, her analysis of Latin liquid dissimilation does not take into account that there are lexical objects in Latin where according to her analysis dissimilation should take place but does not (5)[7] and that the only suffix of the shape -V(:)lis that provides evidence for dissimilation is -aːlis but not *e.g.*-(b)ilis (6) or -ulus, -ula, -ulum (7). A possible argument against my objections is to say that -aːlis, on the one hand, and all other Latin suffixes containing l, on the other hand, differ with respect to their analyticity status.[8] This is to say that either -aːlis is a non-analytic suffix while other suffixes containing l are analytic or *vice versa*. In the former case, -aːlis should behave like any morphologically simplex form, *i.e.* Latin liquid dissimilation should also occur freely with simplex forms. In the latter case, analytic -aːlis should pattern with other analytic forms, all other suffixes containing l with other non-analytic, *e.g.* morphologically simplex, forms. The former scenario (scenario 1) is proven wrong by that fact that there is no liquid dissimilation in morphologically simplex forms (8).[9] Since in the latter scenario

[7]Translations from [293].

[8]According to Kaye [259], a phonologically relevant distinction of morphological complexity takes only into account whether or not the phonology is blind to this complexity and, if it is not blind, what type of structural complexity is involved. Given a stem 'A' and a suffix 'B', there are three possible suffix types relevant here: 1. B might be a non-analytic suffix which results in a form [AB]; in such a case, phonology treats the domain [AB] like [A], *i.e.* like any other morphologically simplex form. Alternatively, B might be an analytic suffix, resulting in 2. [[A]B] or 3. [[A][B]]. Note that is part of the lexical properties of B which type of suffix it is.

[9]For a domain to qualify as simplex in (8), it may consist of more than one domain; however, (8) contains only tokens of (non-dissimilating) l which belong to one and the same morpheme. For example, in line with an analysis of vertiːgoː as vert- (from vert- 'to turn') plus -iːg- '*derivational suffix*' plus -oː '*inflectional suffix*', some might want to analyse loːliːgoː 'squid' as loːl-iːgoː, *i.e.* as [[loːl]iːgoː]. Nevertheless I use this form as an example of a simplex domain because both tokens of l occur in one and the same domain, *i.e.* within loːl-; whether -iːg- and/or -oː is or are part of the same morpheme is not relevant here. Furthermore, Steriade's analysis only takes into account whether or

(scenario 2) -aːlis should pattern with other analytic forms, all suffixes containing l other than -aːlis would have to be analysed as non-analytic forms. Even though this would correctly predict that the suffixes in (6) and (7) as well as simplex forms (8) do not display dissimilation, it would declare the fact that -aːlis is the only suffix exhibiting this process an accident and would also have to neglect the 'exceptions' in (5).

(5)

a. *Stem*	b. *Stem* + -aːlis		c. *Gloss of* b.
lati-	latiaːlis (3)	latiaːris	'of Latium'
fluvi-	fluviaːlis	*fluviaːris	'of or belonging to a river'
lok-	lokaːlis	*lokaːris	'of or belonging to a place'
leːg-	leːgaːlis	*leːgaːris	'legal'
leːt-	leːtaːlis	*leːtaːris	'lethal'

(6)

a. *Stem*	b. *Stem* + -(b)ilis		c. *Gloss of* b.
alt-	altilis	*altiris	'fattened'
eːlekt-	eːlektilis	*eːlektiris	'choice, dainty'
plaus-	plausibilis	*plausibiris	'deserving applause'
laudaː-	laudaːbilis	*laudaːbiris	'praiseworthy'
flaː-	flaːbilis	*flaːbiris	'airy'
ululaː-	ululaːbilis	*ululaːbiris	'howling'
deːlektaː-	deːlektaːbilis	*deːlektaːbiris	'delectable'
laetaː-	laetaːbilis	*laetaːbiris	'joyful'
deːleː-	deːleːbilis	*deːleːbiris	'that may be destroyed'

not a 'phoneme' is lateral; consequently it should not matter whether l or ll is involved.

(7)

a. *Stem*	b. *Stem* + -ulus, -ula, -ulum		c. *Gloss of* b.
klient-	klientulus	*klienturus	'a small or insignificant client'
kolumb-	kolumbulus	*kolumburus	'a little dove'
pil-	pilula	*pilura	'a little ball'
puell-	puellula	*puellura	'a little girl'
diːluːk-	diːluːkulum	*diːluːkurum	'daybreak'

(8)

a. *Simplex domain*		b. *Gloss of* a.
laelius	*laerius	'*name of a* geːns'
lollius	*lorrius	'*name of a* geːns'
laliːsioː	*lariːsio	'fowl of a (variety of) donkey'
liːlium	*liːrium	'lily'
lolliːgoː or loːliːgoː	*lorr-/loːriːgoː	'squid'

Let me illustrate this point with the aid of a diagram as in (4):

(9)

	[+lat]			[−lat]		
	|			|		
mi	l	i	t – a	r	is	*cf.* (4a.)
*	l	o	k – a	r	is	*cf.* (5); predicted dissimilation resulting in -aːris is not attested
*f	l	a	b – i	r	is	*cf.* (6); dissimilation specific to -aːlis
*pi	l		u	r	a	*cf.* (7); dissimilation specific to -aːlis
*	l		ae	r	ius	*cf.* (8); the non-occurrence of dissimilation in simplex stems proves scenario 1 wrong

Even though scenario 2 is at least feasible, there is further evidence against Steriade's account: The second problem of her proposal is that while the existence of the articulatory feature [lateral] and its exclusion from observable sounds like non-liquids and r is supposedly phonetically motivated, the subcategorisation of non-lateral sounds into distinctively *versus* redundantly non-lateral ones is not. The phonetically motivated non-laterality of *e.g.* t and r is by definition identical. Furthermore, looking at all the articulatory features in Steriade's discussion of the distinction of R- and D-values, *i.e.* [high] [pp. 342ff.], [back] [pp. 344ff.], [round] [pp. 347ff.], [ATR] [pp. 349ff.], [lateral] [pp. 351f.] and [voice] [p. 352], there is no case where Steriade can provide phonetic evidence in favour of this distinction.

Note that the phonetic classification of language input is not a pre-theoretical one: which particular phonetic detail is considered to be phonologically relevant depends entirely on the theory classifying it. This is to say that Steriade works within a framework where phonological phenomena are based on and 'scientifically' classified according to properties of the articulatory system but where the units derived from this classification, *i.e.* features, cannot account for the data they are being assumed for. So making the PH in the context of underspecification results in a phonological theory with two modules both of which are assumed in order to explain phonological phenomena: a phonetic module, derived from and motivated by properties of some phonetically defined system and explaining those phonological constraints or processes which it can explain (*e.g.* the non-laterality of r and all non-liquids), and a cognitive module, independent from phonetics and accounting for all cases where the phonetic module is completely oblivious to observable patterns or makes wrong predictions (as in the case of non-lateral r specified as [−lat] *versus* non-lateral non-liquids unspecified for laterality). In other words, the assumption of the PH amounts to an unfalsifiable argument: As long as there are cases where the PH makes some verifiable predictions, it could in Steriade's approach only with difficulties be falsified (*i.e.* it would exhibit an extremely low degree of falsifiability).[10] The cognitive module can always incorporate one more assumption to account for data problematic for the phonetic module.

In the context of the discussion of redundancy rules, it can be said that AP's Default and Complement rules as well as Steriade's proposal of D- and R-values cannot adequately explain phonological data. Both claims presuppose the PH and allow for an unspecified number of stipulations whenever the PH would be proven wrong. The main difference between AP and Steriade is that in AP's framework

[10]Regarding degrees of falsifiability, *cf.* section 1.3.3.

these stipulations are somewhat more language-specific while they are more feature- or phenomenon-specific in Steriade's analysis.

Note also that there are more cases of unfalsifiability as necessary part of underspecification. In the context of underspecified nasality, I would like to point to Cohn [114]. Cohn's support for underspecification becomes obvious in her statement that

> "... it is now generally assumed that some degree of underspecification is used by the phonology" [114, p. 7].

Cohn, comfortable with her mainstream view, simply ignores that which is not 'generally' assumed, apparently confuses science with democracy and, backed up by some majority, does thus not need to provide evidence for underspecification. Interestingly, already in her introduction Cohn makes it clear that her approach is set up as a circular argument:

> "Central questions addressed include the characterization of phonetic vs. phonological rules, formal properties of phonetic rules, and the nature of the phonetic representation for the feature Nasal" [p. 1].

It seems that in Cohn's view, the feature Nasal is a phonological feature on the one hand and is motivated phonetically via its definition as 'nasal' on the other. It therefore makes no sense whatsoever that Cohn would like to find out 'the nature of the phonetic representation for the feature Nasal'. If her approach is to be a scientific one, she cannot assume the relevance of phonetics and at the same time be so non-specific and therefore unfalsifiable that she cannot make predictions about the phonetic nature of the phonological and supposedly phonetically motivated units involved. Now consider the following statement by Cohn:

> "Since the phonological representation alone is not sufficient to determine the phonetic output, it must be concluded that such effects are accounted for in the phonetics. The nature of these effects suggests that they are due to language specific phonetic rules" [113, pp. 66f.].

So if it is possible for Cohn (in [114]) to motivate the 'phonological' feature Nasal phonetically, how can it also be possible for her (in [113]) that, firstly, phonological representations may determine phonetic output and, secondly, that phonetic rules may motivate phonetic output? It seems that for Cohn, phonetics and phonology are intertwined via so many circular arguments that any phonetic as well as any phonological phenomenon may be motivated phonetically and/or phonologically. How could she ever be wrong?

Furthermore, I would like to point out that it is not only obligatory within the phonological mainstream to set up the PH in an unfalsifiable manner but also that the resulting theory-immanent problem of flexibility of applicability (as discussed in section 1.3.2, chapter 1) is commonly stated in such an obvious manner that I find it surprising that the authors in question do not even mention their theories' basic flaw. Let us look at the following two quotes:

> "The claim that two [phonological] representations may have the same phonetic interpretation is not new. Rose (1996) [416] has recently defended this position by demonstrating that phonetic laryngeals are sometimes specified as pharyngeals and sometimes they are placeless segments. For Rice (1996) [410], velars also have variable specification" (Piggott [370, pp. 473f., footnote 15]).

> "Phonology-phonetics mismatches ... provide evidence that phonology is governed by principles that can act independently [*sic*] of the phonetics. This is not to say that phonetics cannot inform the phonology. Phonology can be seen as both related to phonetics and separate from phonetics. ... The existence of tone effects after voiced obstruents has a phonetic motivation. But the actual manifestation of some phonetic effects in the phonology to the virtual exclusion of others argues for a real and significant difference. It also argues that, within the phonological component, phonological forces outweigh phonetic forces when the two conflict" (Bradshaw [63, p. 29]).

Both Piggott and Bradshaw assume that phonetics motivates phonology and that phonology might also be motivated by non-phonetic factors. Furthermore, both authors are not able to predict independently under which circumstances the PH is not to be applied, *i.e.* when phonology can be motivated independently of phonetics. In other words, both researchers support the PH without specifying a single circumstance under which the PH would be proven wrong. Obviously, the PH is unfalsifiable. This, however, means—due to phonetics-independent metatheoretical reasons—that everytime when the PH does make the right predictions, such cases provide no evidence for it.

It is now clear that the PH is an unchangeable credo *in aeternam* which is believed in irrespective of evidence; it is based purely on selective perception, which is why it is one of the main methodological tricks employed by the majority of 'phonologists' to make sure that they are always able to 'account for' some data.[11]

Below, I will present feature geometry (section 2.5) as another unusable method utilised to avoid overgeneration. However, since many phonologists, *e.g.* AP and Steriade, employ underspecification, I will first demonstrate the circularity inherent in the application of the PH to the latter approach in some more detail.

2.3 Underspecification

As I have shown, underspecification is a concept that can only be upheld by making the PH an assumption that is not to be questioned. Note that my arguments were based on the claim that there is no phonetic evidence for underspecification. However, Pulleyblank [403] points to Keating [268] and Pierrehumbert & Beckman [367] who in his opinion provide such phonetic evidence.[12] To be fair, let me discuss both Keating's research and Pulleyblank's application. I will look at Keating's proposal that underspecification does not only play an important role in non-surface forms but also in (phonologically relevant) phonetic representations. I will claim that this proposal is wrong and that phonetic underspecification cannot explain phonological data. Subsequently, I will try to show that Pulleyblank's application of Keating's work to the (presumably) phonological theory of underspecification results in a circular framework which can consequently only confuse the issue rather than add anything to an understanding of phonology.

2.3.1 Keating (1988): underspecification in phonetics

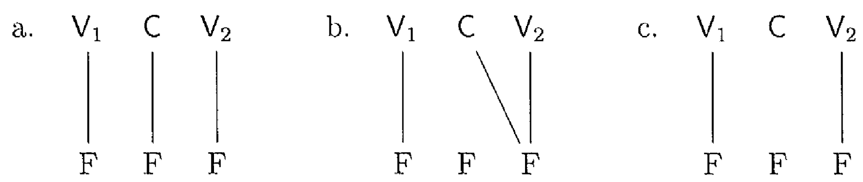
Keating proposes that not only underlying representations but also surface forms may be underspecified and that the presence or absence of feature values in surface forms can be phonetically motivated. For Keating, a major advantage of a phonological theory which allows underspecified surface forms is that "different acoustic patterns correspond to differences in surface feature specifications" [p. 290] and that "immediately accessible surface information could form the basis for an initial distinction

[11] Since due to the apparent lack of evidence for the PH, it has to be believed in, it is also the ideal tool for the acquisition of academico-political power and the establishment of a successful career in phonology and of a great following.

[12] Additional literature, *cf.* Archangeli [14, 15], Browman & Goldstein [70], Clements & Hertz [103, 104], Cohn [114], Pierrehumbert [366], Hertz [214, 215, 216].

between the three representations" in (10).[13] In Keating's view, any segment may correspond to one of these phonological output representations:

(10)



Since in (10a.) feature assignment for a given segment happens independently of adjacent segments, Keating predicts that under these circumstances the phonetic quality of each segment is independent of adjacent segments too so that there can be no vowel-to-vowel co-articulation effects in either direction [p. 287]. In (10b.), the value for F in C is dependent on the value for F in V_2, the following vowel. Therefore Keating expects an effect of V_2 on V_1 due to the effect of the consonant's feature value on V_1. However, she predicts that there may be no co-articulation effects of V_1 on V_2 [*ibid.*]. In (10c.), there should be vowel-to-vowel effects in both directions while the quality of the intermediate consonant will be gradual and transitional [*ibid.*].

In this context, Keating admits that the acoustic surface information assumed to be phonologically relevant only helps the learner of a language to decide whether a segment is underlyingly unspecified for a feature, as for example C in (10c.). It does not contain any information about whether an acoustic property of a segment is due to surface or underlying feature assignment. The consonant in (10a.), for example, could be specified for F underlyingly or not. In Keating's own words:

> "The phonology of the language system (the system of contrasts, behaviour of the segment in other rules) still has to be consulted to discover the source of the feature value" [p. 287].

The information in the acoustic patterns corresponding to the three representations in (10)

[13]The representations in (10) are taken from Keating [268, p. 287].

"only reflects surface specification *vs.* underspecification, and therefore provides evidence only about what could have been originally unspecified. Additional analysis by the learner would be required to arrive at appropriate underlying representations" [pp. 290f.].

Clearly, there is virtually nothing in this view which enables phonologists or a child acquiring a language to decide which part of the phonetic input is underlyingly, *i.e.* lexically, relevant. Even the differentiation between surface specification *versus* underspecification contains no phonologically relevant information: On the one hand, acoustic properties of a segment are assumed to be phonologically relevant in that they contain information about surface representations—acoustic phonetics motivates phonology. On the other hand, underlying representations are assumed to be deductible not from vowel-to-vowel co-articulation effects, *i.e.* one type of phonetic evidence, but from some other type of phonetic evidence. The latter type is elicited via a consultation of the system of contrasts and rules of a language where both system and rules are (presumably) defined via units like the underlying feature [nasal] which are also phonetically motivated but by a different kind of phonetic evidence. In other words, Keating assumes underlying representations which are partly phonetically motivated via consultation of the phonetically classified system and partly independently of phonetic evidence via disregard of transparency effects. Since surface forms are derived from underlying forms they are phonetically motivated via system consultation; additionally they are also motivated via transparency effects. Consequently, surface forms are somehow more phonetically motivated than underlying ones. Since Keating does not provide an independently established criterion according to which one could decide objectively what type of phonetic evidence is employed to motivate surface forms and, more importantly, what type of phonetic evidence cannot be used to motivate underlying forms, this state of affairs is not desirable at all: In Keating's approach, acoustic phonetics does motivate phonology, does not and varies with respect to the extent to which it does it, all within the same framework. Unfortunately, this problem is not discussed by Keating (strategy 1: denial).

To sum up, Keating's proposal results in a model which contains the PH as an assumption that is deemed acceptable independently of how often, in what way or under which circumstances it applies. If follows that in Keating's framework the PH cannot be falsified and that any phonological theory which incorporates Keating's proposal is fundamentally mythological even if it does not create further problems.

2.3.2 Pulleyblank (1995) completes the circle: phonetics in underspecification

Due to the highly questionable status of Keating's findings, it is interesting that Pulleyblank [403] tries to explain Keating's phonetic data by underspecification theory. In a section named "Underspecification in the phonetics" [pp. 14ff.], Pulleyblank states that "arguments for not specifying a particular feature can be of various types: transparency effects, phonological inertness, distributional asymmetries, and so on" [p. 14].[14] In this context, Pulleyblank considers "phonological transparency" to be a "compelling argument" [*ibid.*][15] since, in his view, there are phonetic facts which "receive a straightforward explanation in a theory incorporating underspecification" [p. 15]. In a string as in (11) [*ibid.*], A and C are specified for some value of a feature while B is not:

(11) *Transparency*

$$\begin{array}{cccc} A & B & C & \\ | & & | & \\ \alpha F & F & \beta F & F - \text{tier} \end{array}$$

As I have pointed out above, Keating provides evidence that shows that there are vowel-to-vowel co-articulation effects which can occur without being influenced by an intervening consonant. The formants of *e.g.* A in (11) may gradually change into those of C where the consonant in B has no effect on this change. Pulleyblank's 'straightforward explanation' is as follows:

> "Since 'B' is unspecified for feature F in the output of the phonological component, there is nothing to interfere with the phonetic coarticulation of 'αF' and 'βF' ." [p. 15]

In other words, phonetic co-articulation effects are dependent on (the output of) the phonological component. On the other hand, phonological processes are motivated

[14] Pulleyblank [p. 16] refers to Mohanan [329], AP (in press) [*sic*] and references therein for the relevant evidence. In Durand & Katamba's [148] bibliography there is no entry under "Archangeli & Pulleyblank (in press)".

[15] For an example of a possibly compelling transparency effect, *cf.* Steriade's analysis of Latin liquid dissimilation above (section 2.2.2.2).

by or grounded in, *i.e.* dependent on, phonetics and the phonetics involved form an essential part of the definition of the units employed in underspecification. So not only is Pulleyblank's underspecification in the phonetics, the phonetics is also in the underspecification. Obviously, Pulleyblank's argument is circular and the 'phonetic evidence' in favour of underspecification theory is not independent and is therefore immaterial. Like any other version of the PH, underspecification is not theoretically falsifiable and is thus not an empirical assumption. I conclude that underspecification of phonetically defined units is phonologically irrelevant.

2.4 A word about monovalent features

In a feature theory with exclusively monovalent features (*cf.* Anderson & Ewen [6], Ewen & van der Hulst [155], van der Hulst [227, 228], Avery & Rice [21]), the fact that assimilatory processes can only or at least more often be observed for one of the values of a feature is actually predicted by these frameworks and cannot be turned into an argument against them. However, Pulleyblank's [403, p. 32] observation that some features are more commonly employed cross-linguistically than others (*cf.* section 2.5.1) is a problem that monovalent, non-monovalent and mixed systems which make the PH cannot solve. Neglecting that a monovalent feature theory shows a lower degree of overgeneration, all of the problems the PH entails remain unsolved in a monovalent theory. For example, the non-existence of a well-defined phonetic framework and of a phonetic definition of supposedly phonetically defined notions like 'nasality' (*cf.* chapter 1) make any attempt to incorporate the PH into a phonological framework futile independently of the valency or hierarchical order of the units in question.

2.5 Feature geometry

The final type of feature co-occurrence restriction I would like to look at is feature geometry (*cf.* Clements [100], Sagey [424], McCarthy [320], Pulleyblank [403]). I will choose Pulleyblank [*ibid.*] to illustrate the basic assumptions made within this framework and to show in what way feature geometry like underspecification or language- or feature-specific monovalency or redundancy rules results in a framework where the assumption of the PH can only be upheld by making it unfalsifiable (*cf.* section 2.5.1). In section 2.5.2 I will look at Piggott's [369] application of feature geometry to the study of nasal harmony phenomena and illustrate in what way this move is responsible for a number of explanatory inadequacies because of which I will ultimately reject Piggott's attempt.

2.5.1 Pulleyblank (1995): motivation for and basic concepts of feature geometry

Pulleyblank starts his argumentation for feature geometry by pointing to the shortcomings of linear phonological models and of autosegmental theory. Linear[16] approaches like Chomsky & Halle [97] ('SPE') "viewed segments as composed of unordered sets of binary distinctive features" [403, p. 4]. Pulleyblank [*ibid.*] provides the following matrices to clarify this point:

(12) *Linear models*

$$\begin{bmatrix} \alpha\mathrm{F} \\ -\beta\mathrm{G} \\ \gamma\mathrm{H} \\ \delta\mathrm{I} \\ \vdots \end{bmatrix} \quad \begin{bmatrix} -\alpha\mathrm{F} \\ \beta\mathrm{G} \\ -\gamma\mathrm{H} \\ \delta\mathrm{I} \\ \vdots \end{bmatrix} \quad \begin{bmatrix} \alpha\mathrm{F} \\ \beta\mathrm{G} \\ -\gamma\mathrm{H} \\ \delta\mathrm{I} \\ \vdots \end{bmatrix}$$

Such linear models have two main disadvantages: they predict that dissimilation phenomena should be as common as assimilation processes and similarly, homorganic and non-homorganic place assimilation should occur equally frequently. To show that rules within a linear model predict and account for non-homorganic place assimilation and in fact value it equally in its formalism, Pulleyblank [p. 6] gives the following example:

(13) *Impossible place assimilation pattern*

$$\begin{bmatrix} +\text{ consonantal} \\ +\text{ nasal} \end{bmatrix} \longrightarrow \begin{bmatrix} \beta\text{ coronal} \\ \alpha\text{ anterior} \\ -\gamma\text{ labial} \end{bmatrix} \quad / \_\_ \quad \begin{bmatrix} \alpha\text{ coronal} \\ \beta\text{ anterior} \\ \gamma\text{ labial} \end{bmatrix}$$

The first prediction is proven wrong by the fact that assimilation is much more widespread than dissimilation. The second prediction is discredited by the non-existence

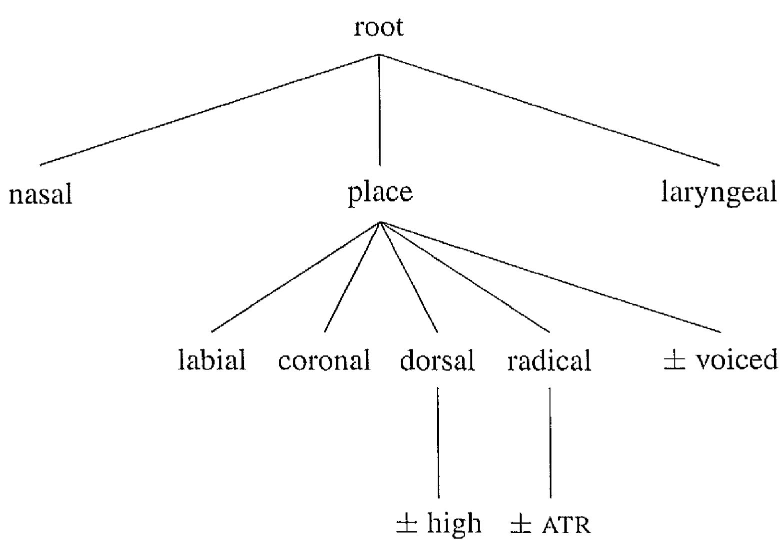
[16] Pulleyblank [p. 4] has: "Non[*sic*]-linear approaches to feature content such as SPE viewed segments as composed of unordered sets of binary distinctive features [...]" The diagram in (12) taken from the same page and Pulleyblank's next paragraph starting with "Such a linear model of segmental structure" make it clear that 'non-linear' is to be substituted by 'linear'.

of non-homorganic place assimilation phenomena; for example, nasal place assimilation might produce [m + b] or [n + d] but never [m + j] or [ñ + b] [pp. 5ff.].

In his discussion of the short-comings of autosegmental models,[17] Pulleyblank notes that, opposed to linear models, autosegmental theory does not predict cases of non-homorganic place assimilation as in (13). So in order to explain nasal place assimilation, an autosegmental model would assign the appropriate, *i.e.* assimilating, place features to an independent tier. For example, in a nasal-obstruent sequence, the obstruent would be lexically specified for the relevant place features while the preceding nasal would be unspecified for them. Phonological derivation would then spread these features leftward from obstruent to nasal. However, Pulleyblank [p. 7] points out that in (standard) autosegmental theory any feature combination can be assigned to a particular tier. Therefore it predicts like linear models incorrectly that "a cross-linguistic survey of assimilation processes should demonstrate any and every possible combination of features as a legitimate assimilation process" [*ibid.*]. Since only certain types of assimilation phenomena do occur, Pulleyblank argues for "restricting assimilation rules to those involving narrowly delimited classes of feature sets" [p. 8].

The method of restriction Pulleyblank opts for is feature geometry. In this framework, features are ordered in a hierarchical manner as in figure 2.1 [p. 14] (p. 74).

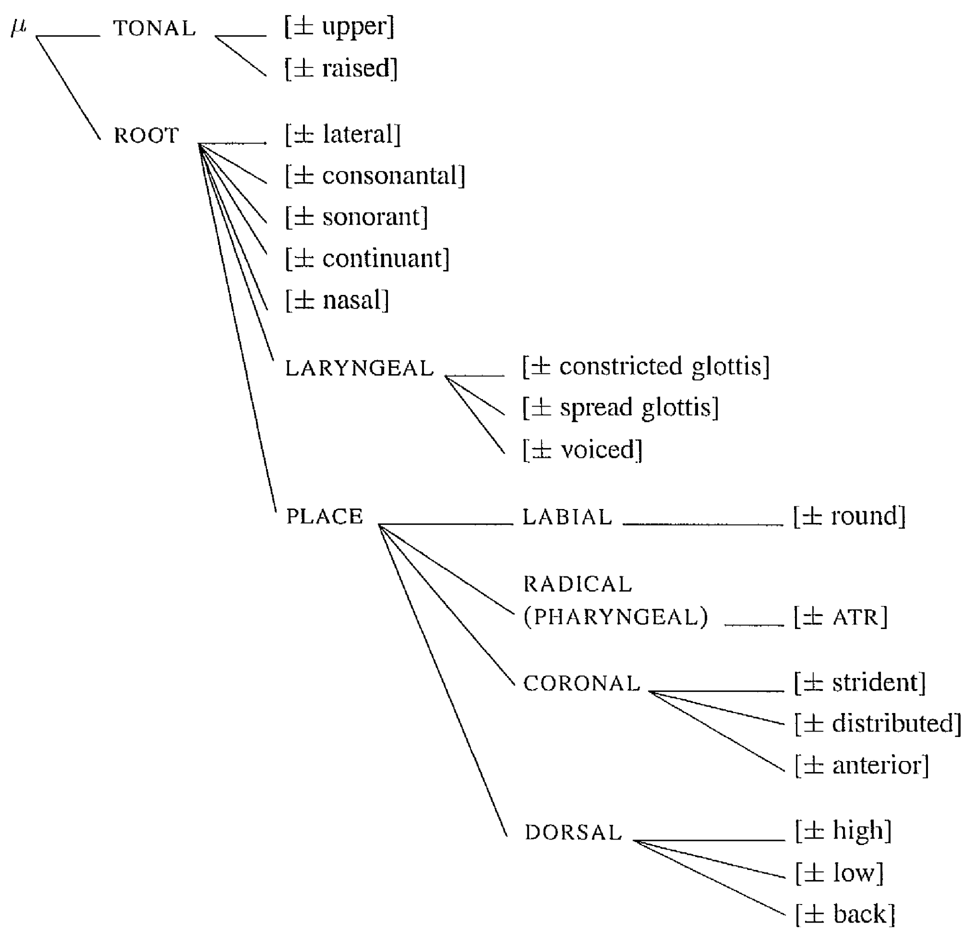
Figure 2.1: The geometry of terminal features (Pulleyblank [403])



[17] Pulleyblank refers to Goldsmith [177] and Hayes [210].

The illustration in figure 2.1 is highly selective. In order to let the reader appreciate a more realistic scale of the complexity involved in feature geometry, let me also provide the system Pulleyblank subscribes to in AP [17, pp. 18–25] (*cf.* figure 2.2, p. 75):[18]

Figure 2.2: Feature geometry according to Archangeli & Pulleyblank [17]



As illustrated in figure 2.2, class nodes are monovalent while terminal features are binary in Pulleyblank's view. (Unary) nodes are each assigned their own tier and, neglecting language-specific constraints, can thus combine freely. The complement class of a class defined by a class node is not a natural class. The two values of terminal (binary) features, *e.g.* [+ATR] and [−ATR], are assigned the same tier and may therefore not co-occur. Both values of a terminal feature define a natural class. Fur-

[18] The illustration in figure 2.1 is in Pulleyblank's [403] spirit. The drawing he provides [p. 14] only contains some of the class nodes and terminal features shown in figure 2.2. This more complete drawing is an adapted version of an illustration in AP [17, p. 20] which in turn is after Sagey [424].

thermore, class nodes must be underspecified while terminal features show a degree of variation with respect to which value is "phonologically active" [p. 31]. In AP [17, p. 21], Pulleyblank agrees with Sagey [424] in allowing content-bearing class nodes, *e.g.* Coronal, to be terminal. However, AP [*ibid.*] propose in disagreement with Sagey [*ibid.*] and Avery & Rice [21] "that all terminal nodes must correspond to phonetic content". Thus "purely structural nodes like Laryngeal and Place" [17, *ibid.*] may never be terminal but have to dominate feature content.

As I will try to show, feature geometry is—due to the presupposition of the PH—fundamentally flawed. One problem Pulleyblank's theory has is that articulatory phonetics motivates an essential part of the definition of class nodes and terminal features without making any predictions about valency or the feature content of terminal nodes. In other words, there is no evidence provided by articulatory phonetics which supports the assumption of either monovalency, binarity or multivalency, or the restriction of terminal nodes to those with feature content. The evidence for the proposal of unary class nodes *versus* binary terminal features as well as the arguments for the restriction of terminal nodes to those having feature content come exclusively from Pulleyblank's or other phonologists' analyses of phonological phenomena. I am not suggesting that there is anything wrong in establishing evidence based on research of many different subjects since this approach could increase the amount of independent evidence significantly. However, in Pulleyblank's version of feature geometry it is necessary as in Steriade's case (*cf.* section 2.2.2.2) to create a framework containing two co-dependent, not independent, modules, a phonetic and a cognitive one: the definition of nodes and features is motivated by articulatory phonetics, their respective valency is motivated independently. Similarly, Laryngeal—which cannot occur terminally—is just as much phonetically motivated as Labial—which may occur terminally.

So in this theory, the phonetic and the cognitive module are not responsible for the same kind of evidence. It follows that phonetic and cognitive module do not support each other as they would if they were to provide independent evidence for each other. The cognitive module, partly defined in terms belonging to the phonetic module, can never deliver independent evidence for the phonetic module because it accounts for all those cases which cannot be motivated by phonetic evidence. Obviously, the assumption of the PH forces its supporters to create a framework where phonetic and cognitive module are co-dependent. Since assumptions within the phonetic module are based on properties of some phonetically defined system which in turn can be measured independently (*e.g.* via electropalatographic or spectographic analysis)[19] while properties of the cognitive module are entirely theory-dependent, it is the cognitive module which has to contain the bulk of stipulations necessary. For example, since the applicability of the phonetic notions incorporated by nodes and features, like [dorsal] or [±ATR], is (supposedly) phonetically motivated, the very choice of the features involved and the different types of valency of nodes and features has to be stipulated within the cognitive module as long as no phonetic evidence can be found to support this claim. This is to say that as long as it is possible to 'correct' problems of the PH within the cognitive module, the PH is not falsifiable.

Consequently, a closer look at Pulleyblank's distinction of monovalency for class nodes and binarity for terminal features and the ill-formedness of terminal nodes without feature content as proposed by AP [*ibid.*] reveals that the PH as part of Pulleyblank's version of feature geometry is set up as an unfalsifiable credo, not as a scientific assumption. So-called 'empirical' evidence in favour of any of Pulleyblank's distinctions does not make the PH contained in his approach even a fraction more falsifiable.

The second problem of feature geometry is that "some class nodes are more commonly involved in establishing oppositions than others" [p. 32]. Pulleyblank provides a number of examples [*ibid.*]: The tongue root is used less frequently than the tongue dorsum. Also, laryngeal oppositions do not occur universally. Furthermore, "invoking values like [+back], [+voiced], [+high] is very common, while invoking a feature value like [+ATR] is much less so" [*ibid.*]. Pulleyblank provides the following explanation for these differences in frequency: He wants "to propose only those feature specifications required to establish the oppositions attested in a particular language, filling out such representations with redundant specifications to the extent that there is positive evidence for such" [pp. 31ff.]. In his view, this predicts the specification of features to be "sparse, at least at initial stages of representation" [p. 32]. The varying occurrence frequencies of different nodes as described above are added without further comment. Neglecting the theoretical possibility that the observation of varying frequencies is independent of the proposed restriction of feature specifications to those necessary in a particular language, this must mean that Pulleyblank explains the attested frequency variations by the predicted sparseness of feature specifications. However, he does not clarify in what way sparseness can be

[19]For an example of a study employing electropalatographic and spectographic data, *cf.* Shin & Hayward [445, 446] and the relevant references therein.

measured and why it would be particularly prominent in cases like [+ATR] as opposed to [+back], [+voiced] or [+high]. In other words, Pulleyblank does not have an explanation for the observed frequency variations. Furthermore, since there is no phonetic evidence for these variations of phonetically motivated classes and nodes, an explanation for them could in Pulleyblank's framework only be derived within the cognitive module. As discussed above, this would be empirically unsound due to the resulting unfalsifiability of the PH.

A further problem of feature geometry is one that feature geometry shares with all phonological theories that make the PH: Concepts like Coronal, Labial, Nasal, *etc.*, are assumed to be relevant to phonology. As I have demonstrated using the example of nasality, such concepts are not based on a well-defined phonetic framework at all but rather so vague (and hence unfalsifiable) that I find it surprising that anyone professionally interested in phonology would even consider making them part of their framework.

In light of this evidence, it remains unclear why Pulleyblank follows Sagey's [424], Ladefoged & Halle's [284] and Ladefoged's [283] proposal that "the features that define and play a role in phonological processes bear a direct relation to physiological properties of the vocal tract and to acoustic properties of the speech signal" [403, p. 8]. Furthermore, Pulleyblank's view that "there must [*sic*] be features defining movement in areas such as ... labial, coronal, dorsal, radical, laryngeal, nasal" [*ibid.*] turns out to be completely unfounded.

2.5.2 Piggott's (1992) analysis of nasal harmony within feature geometry

Piggott subcategorises cross-linguistically observable nasal harmony phenomena into two harmony systems and one fusional system ("pseudo-harmony" [p. 35]). To illustrate the failure of Piggott's feature geometry based approach, I will only discuss some of Piggott's predictions concerning the first two harmonic systems which I will refer to as types 'A' and 'B'. I will also present the difference between type A and B languages in my own terms and point to a number of observations and languages not mentioned by Piggott. The reason for this is that, as I will discuss below, I do not consider Piggott's representation adequate.

According to Piggott, type A harmonies can be found in Warao (*cf.* Osborn [358]), Capanahua (*cf.* Loos [305], Safir [422, 423]) and some Malayo-Polynesian languages. Type B harmonies occur in a number of Amerindian languages of South-

America including many languages of the Tupí and Tucanoan families, *e.g.* Southern Barasano (*cf.* Smith & Smith [450]), an Eastern Tucanoan language. Consider the following data:[20]

(14)

Type A: Warao		*Type B: Southern Barasano*	
inãw̃ãh̃ã	'summer'	mãh̃ã́ŋɨ̃	'comer'
mõj̃õ	'cormorant'	ã́j̃ã	'a snake'
hĩh̃ã	'kind of bird'	ẽh̃ṍ	'flu'
mõãũ	'give it to him!'	ŋã́mṍr̃õr̃ĩ	'ear'
mõãũpu	'give them to him!'	mãsã́	'people'
mẽh̃õkohi	'shadow'	kã́mõkã́	'a rattle'
tewẽke	'kind of bird'	w̃ãtĩ́	'demon'
terẽ	'it broke'	r̃ĩã́	'children'
jã	'walking'	j̃ũkã́	'a drink'
etere	'bell'	ᵐbári	'food'
pajara	'sword fish'	wája	'come!'
hiha	'your hammock'	eháre	'arrival'
oi	'look out!'	riá	'egg'
ja	'sun'	juká	'vulture'

Piggott analyses Warao and Southern Barasano (henceforth 'SB') as left-to-right harmonies. One of the main differences is that while in SB (type B) there are no blocking expressions, in Warao (type A) only w, j, h and vowels are transparent and do not block nasal spread.[21] Furthermore, both type A and B languages have a series of neutral stops and one of nasal stops; however, while type A languages may have a series of voiced stops—prenasalised in some languages—type B languages must have this

[20]I give more data in the following table than Piggott does and partly, particularly for Southern Barasano, I use different examples; his and my sources are identical though. This difference is not relevant here. Neglecting that Piggott does not indicate pitch, he has "mãh̲ãŋĩ̲" [p. 46]; according to my copy of Smith & Smith [450, p. 82] plus their comments about "[h̥]" [p. 83], *i.e.* the nasalised version of h, he must be mistaken. Note also that Smith & Smith [p. 83] have ň instead of r̃ and use ň only as the nasalised version of r. Additionally, their phonemic form shows high pitch on the final a of r̃ĩã́, while their phonetic form in square brackets does not.

[21]In languages of type A which, as opposed to Warao, have intervocalic ʔ, ʔ behaves like w, j, h and vowels and is always transparent and a landing site for nasal spreading. Note also that the SB data in (14) does not help to clarify whether nasality in this case spreads leftwards or, as Piggott assumes, rightwards. Piggott [pp. 47–55] discusses the directionality of SB harmony. For the purposes of this section, I will agree with the assumed directionality (left to right).

additional series. Also, while in type A languages the series of voiced stops might or might not be in complementary distribution with the nasal series, in type B languages it always is. If these two series are in complementary distribution in a language, the voiced stops are obligatorily followed by an OV, while nasal stops always precede NVs.

Piggott does not discuss that transparent expressions ('glides') behave similarly in this respect: some languages exhibit the constraint that transparent expressions must be oral when preceding OVs, but nasal when followed by NVs. However, if nasal/oral agreement occurs between alternating nasal *versus* voiced stops, this implies the occurrence of the same kind of agreement between nasalised and oral glides, but not *vice versa*. For example, in Guaraní (type B) there is nasal/oral consonant alternation between nasal and voiced stops dependent on the following vowel *and also* between nasal and oral glides (21) while there is no language which like Guaraní displays nasal/oral alternation between nasal and oral stops but unlike Guaraní not also between nasal and oral glides. In Secoya (type A), on the other hand, there is no series of voiced stops[22] and thus no alternation between nasal and voiced stops; transparent expressions, however, do exhibit nasal/oral alternation dependent on the following vowel (j̰ḗj̃ḗ (19a.) *versus* jáhí (19b.)). Note that even though transparent expressions in type A languages like Warao and Secoya always let nasality 'pass through', thus being harmonised themselves, they might (Secoya) or might not (Warao) take part in nasal/oral assimilation between onset and following nucleus. It follows from the above that in type B languages—in which nasal and voiced stops must be in complementary distribution—glides must take part in nasal/oral agreement too, *i.e.* nasalised and oral glides must be in complementary distribution.

For example, SB (type B), as mentioned, displays left-to-right nasal harmony; it also has a series of voiced stops which is in complementary distribution with the nasal stops series. This implies that w and j are nasalised in SB when preceding a nasal vowel in an environment in which they could not have been nasalised from the left (w̃ãtĩ́, j̃ũkã́). Warao does on the other hand not have voiced stops, so there is of course no alternation between nasal and voiced stops. For a language like Warao it is thus possible to have glides which, when not nasalised via nasal spreading, can

[22] According to Johnson & Peeke [241], Secoya has the consonant inventory: p t ʧ k kʷ ʔ d s m n w j h. Their symbols for ʧ ʔ j are "č ˀ y" [241, p. 81]. d is classified by them as "alveolar voiced stop". Since there is no other 'voiced stop' nor any type of r, there is in my opinion no evidence in the Secoya data provided which suggests the proposal of any D-expressions (*cf.* (15)); what Johnson & Peeke, misguided by the PH, see as voiced stop is in my view simply the Secoya version of r. Since r in Secoya does not get nasalised when preceding a NV, it is clearly a T-expression here (*cf.* (15)).

be oral when followed by NVs. The relevant Warao examples are words like tew̲ẽke (*tew̲̃ẽke) or j̲ã (*j̲̃ã).

To clarify these points not discussed by Piggott but important to understand the typological pattern involved, let me point out here that there are three relevant types of consonants in *all* type B languages and they can be summed up as in (15). Note that there are languages, *e.g.* Cubeo (*cf.* section 1.5.2 and Salser [425]), which display the consonantal alternations in (15) but where the domain for orality or nasality agreement is categorically precisely one onset-nucleus pair ('ON' pair/sequence), *i.e.* nasality never spreads beyond one ON-sequence. Let me call this agreement between a nucleus and its (preceding) onset *nasal sharing*.[23] So based on the above it can be said that type B languages (which are languages where nasality spreads through a whole domain (if not blocked), *i.e.* beyond one ON-pair) always also exhibit nasal sharing.:

(15) *Nasal sharing: a must in type B languages*

Expression	*Pattern*	/_Ṽ		/_V	
T-expressions	T	TṼ	tã	TV	ta
N-, D-expressions	N ~ $^{(N)}$D	NṼ	nã	$^{(N)}$DV	$^{(n)}$da
G-expressions	G̃ ~ G	G̃Ṽ	j̃ã	GV	ja

Moreover, Piggott [p. 47] mentions that this alternation can also be found in suffixes (*cf.* (16)). For example, in SB the infinitive suffix has two forms, -re or -r̃ẽ. Since SB stems must end in a vowel, this suffix is always preceded by an immediately adjacent vowel. If such a stem-final vowel is oral, -re is selected, if it is nasal, the infinitive suffix is -r̃ẽ.[24] In other words, in SB, a language with left-to-right nasal spreading, (some) suffixes can be harmonised. Similarly, in Desano (*cf.* Kaye [249, 250]), an Eastern Tucanoan language not mentioned by Piggott, there is a suffix -ru alternating with -nũ ('classifier for round or hollow objects'); again, the selection is dependent on the orality/nasality of the preceding stem: after oral stems, -ru is affixed, after nasal stems, -nũ. Note that in SB, there is an expression r alternating

[23] I will analyse nasal sharing as 'L-licensing' in chapter 7.

[24] Piggott [p. 47] seems to assume that the nasalised version of re is nẽ. His and my source, Smith & Smith [450, p. 83] state: "The voiced alveolar flap is /r/ which has a variant [ň] that occurs in the environment of nasalised vowels". Note that I use r̃ for ň. However, Smith & Smith [p. 82] themselves use the examples which can be found in Piggott and which obviously mistakenly have -nẽ instead of -ňẽ (r̃ẽ). Furthermore, Piggott [*ibid.*] also gives -mã 'future imperative' (identical in Smith & Smith [p. 82]) as the nasalised form of -^{m}ba, which is -^{m}bá (with high pitch, '(non-future) imperative') in Smith & Smith [*ibid.*]. I will therefore omit Piggott's 'imperative' suffix -mã ~ -^{m}ba.

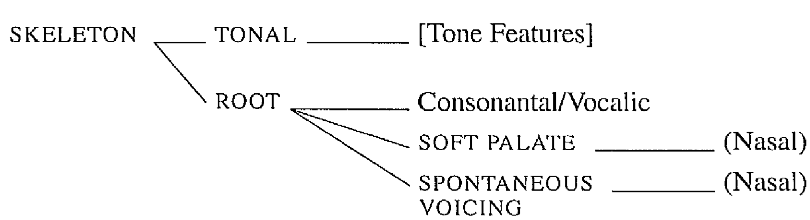
with r̃ and additionally an expression d alternating with n. In Desano, there is no r which is distinct from d. Following Kaye [*ibid.*] I assume Desano r to be in a phonological class with b and g, or, in my terms, to be a D-expression; SB r, on the other hand, I consider to be a G-expression, *i.e.* to be in a class with j, w and h. In the Desano examples mĩ-nĩ-nũ *versus* wɨa-ri-ru in (16), the infinitive marker -ri (alternating with -nĩ) is another suffix whose orality/nasality is dependent on the preceding stem:[25]

(16) *Suffixes in type B languages*

	a. *Nasal stems*		b. *Oral stems*	
SB	h̃ṹnĩ-r̃ẽ	'to hurt'	idí-re	'to drink'
	j̃ãŋṍ-r̃ẽ	'to speak'	ahé-re	'to play'
Desano	sẽnã-nũ	'pineapple'	go-ru	'ball'
	mĩ-nĩ-nũ	'a small round thing'	wɨa-ri-ru	'a large round thing'

To account for these types of nasal harmonies, Piggott [p. 49] proposes the hierarchical ordering of [nasal] shown in figure 2.3 (p. 82), which is different from the version shown in figure 2.2 (p. 75).

Figure 2.3: The hierarchical ordering of [nasal] according to Piggott [369]



In Piggott's view, the feature [nasal] is dependent on the Soft Palate node in type A languages but on the Spontaneous Voicing node in type B languages. The Spontaneous Voicing node is phonetically 'motivated' as node for sonorancy. Furthermore, he claims that it is the Soft Palate node dominating [nasal] which spreads in type A languages, while in type B languages the feature [nasal] itself, linked to Spontaneous

[25]In j̃ãŋṍ-r̃ẽ, Smith & Smith [450, p. 82] have high pitch on the nasalised o in their phonemic form but on the nasalised e in their surface representation. Since the infinitive suffix does not carry high pitch in other forms, I assume high pitch on e constitutes a typo.

Voicing, spreads through a harmony domain from sonorant to sonorant.

The arguments for this re-arrangement of the feature [nasal] within the geometrical hierarchy are dubious. In Piggott [369, p. 37], [nasal] is dominated by the Soft Palate node which in turn is dominated by the Root node. This ordering is according to Piggott "argued for in Piggott (1987) [*i.e.* in [368]]" [369, p. 37]. However, in Piggott [368, p. 229], *i.e.* Piggott's modification of Clements' [100] original, "the feature nasal is linked directly to the root node". Neglecting Piggott's problems with different versions of his proposal, the approach discussed here [369] predicts in his own view that the harmony pattern of type A languages "can only be triggered by a consonant" [p. 34], while in languages of type B all sonorants may be triggers. Piggott's account also predicts the observable fact that, as demonstrated above, there are no opaque and no blocking 'segments' (Piggott's term) in type B, while in type A languages some consonants are opaque segments and only non-continuant obstruents may block spreading. Which expressions are opaque or transparent or block nasal harmony is in Piggott's analysis subject to further language-specific constraints.

There is unfortunately not enough space here to discuss Piggott's article in detail but let me present the main arguments against Piggott's account. I will show that his claims about nasal harmony are *ad hoc*, ignore typological evidence and make a number of predictions which are wrong to such an extent that any attempt to save this feature geometry based approach will appear futile.

Firstly, as I have demonstrated in section 2.5.1, feature geometry—popular as it might be—is simply not a scientific approach to phonology because one of its key assumptions, an articulatory version of the PH, is set up in an unfalsifiable manner. Therefore any analysis within this framework, even if it happens not to make any further wrong predictions, must be rejected. Consequently, when Piggott [p. 34] states that the "analysis of nasal harmony presented in this paper assumes the fundamental correctness of the segment structure outlined by Clements (1985), Sagey (1986) and others", he automatically allocates his article to the domain of phonological irrelevance.

Secondly, even if one were to neglect the basic problems with empiricism inherent in feature geometry, Piggott's approach contains an *ad hoc* 'explanation' because the feature [nasal] is the only one that to Piggott's knowledge "may be organized under more than one node" [p. 75]. Piggott justifies this by pointing out that this "may be a reflection of the fact that [nasal] is neither a structure feature nor is it correlated with the action of an articulator; it is strictly related to airflow" [*ibid.*].

Assuming that nasality is only related to airflow and that Piggott's theory is motivated by articulatory phonetics, there should not be a phonological concept of phonetically motivated nasality if all other features are correlated to structure or articulator actions. As I have shown in chapter 1 though, the claim that the concept of phonological 'nasality' is phonetically motivated has to be so vague that it does not even matter whether an articulator responsible for the phonetic manifestation of this concept can be found: a strict relation to airflow, not an articulator, will do for Piggott as well as for most other phonologists. Moreover, in chapter 1 I have also provided evidence which makes it abundantly obvious that the feature [nasal] is *not* strictly related to airflow. To sum up this argument against Piggott's account, even if one ignores the general problems of the articulatory hypothesis—and why should one?—, Piggott's version of feature geometry is not phonetically motivated even though his chosen framework supposedly is (but really is not). Additionally, his modifications increase the degree of stipulation within feature geometry. Piggott's claim that his proposal is a "truly revolutionary idea" [p. 74] turns out to be extraordinarily accurate.

My third argument argument against Piggott's account is concerned with the consonantal patterns displayed in (15) which can be found in Cubeo and all type B languages. To explain the data in question, Piggott [p. 55] provides a rule, which he calls "voice fusion", that fuses Spontaneous Voicing nodes within a syllable in a way such that the nucleus of a syllable dominates the left-adjacent onset. He formulates this voice fusion rule for SB [p. 55] and for Guaraní [p. 60] in the following way:

(17) *Voice fusion*

> "SV-nodes [*i.e.* Spontaneous Voicing nodes] are fused within a syllable; the features of the right node (*i.e.* the nucleus or the head of the syllable) dominate."

In his view this predicts that oral sonorants are followed by OVs while nasal sonorants are followed by nasal vowels. However, even though one would expect that it is possible for Piggott to motivate independently via the science of articulatory phonetics which consonantal segments are specified for Spontaneous Voicing, Piggott has to look at the phonetically unmotivated part of the phonologies of the languages involved and stipulate for each of the languages discussed by him which segments are specified for Spontaneous Voicing and which ones are not. Articulatory phonetics does obviously not even provide the information necessary to establish a correlation between major segment classes and Spontaneous Voicing. For example, Piggott is not able to provide articulatory evidence for predictions about which of the segment classes 'obstruents', 'laryngeals', 'sonorants', and so forth, may or must be specified for Spontaneous Voicing [pp. 57f.]. This shows that either the proposal of Spontaneous Voice fusion, that of phonetically motivated segment classes or both of these claims are phonologically useless.

Fourthly, in Piggott's [p. 34] own words, "Since only [+consonantal] segments are underlyingly specified for such a node [*i.e.* the Soft Palate node], this [type A] harmony pattern can only be triggered by a consonant". Let me remind the reader that Piggott analyses Warao as a type A language with nasal harmony spreading from left to right. Piggott therefore predicts that there should be no word in Warao which starts with a NV since the nasality on that vowel should have been triggered by a consonant to its left (18a.). Similarly, a NV should not be preceded by an oral glide (18b.). Moreover, a blocking expression, *e.g.* a voiceless obstruent, should never be followed by a NV (18c.):

(18) *Type A data problematic for Piggott: Warao*

a.	ũĩ	'angoleta bird'
	ĩõ	'kind of turtle'
	õkõ	'kind of bird'
b.	hĩh̃ã	'kind of bird'
	jã	'walking'
	tewẽke	'kind of bird'
c.	terẽ	'it broke'
	sãĩ	'sound of talking'
	tãẽ	'it fell'

More generally, if Piggott's account for type A languages holds true, a Warao NV should be preceded by an obligatory NC independently of whether the nasality of this consonant is underlying or derived. Piggott does not discuss this prediction nor does he mention that data detrimental to his analysis does exist. Let me add here that there is no series of voiced stops (D-expressions) in Warao; also, since r is opaque and blocks nasal spreading, I consider it to be in a phonological class with obstruents (18c). Since r can hardly be phonetically motivated as voiceless obstruent this is an excellent argument against the phonological relevance of articulatory motivated notions like 'obstruent' and 'voiceless'. In (18a.), õkõ has to be mentioned as a maybe somewhat awkward word. Since nasality is blocked by all consonants apart from w, j, h (*e.g.* by k in tewẽke, *tewẽkẽ), it has to be assumed that õkõ is either one domain with two underlying nasal nuclei, according to Osborn's [358] data a rare domain type in Warao, or that it is morphologically complex consisting of either two [[õ]kõ] or three [[õ][kõ]] domains.[26] However, *all* of the data in (18) contradict two interdependent predictions by Piggott about type A languages: not only is he wrong in claiming that only consonants may be underlyingly nasal; opposed to his view, nasal harmony of type A may not only be triggered by consonants but also by vowels.

This is not to say that there are no type A languages as Piggott predicts them. For example, those languages which exhibit type A spreading of nasality, *i.e.* harmony blocked by certain consonants, and which simultaneously disallow NVs preceded by OCs in left-to-right harmonies, do exist: Piggott [p. 41] points to the Sundanese data in Robins [413], which appears to display rightward type A spreading but no word-initial NVs or other counter-evidence. However, since there are more type A languages like Warao which contradict Piggott, the dichotomy between type A and type B languages as proposed by him cannot account for human languages. To support this further, let me provide some data from Secoya, a Western Tucanoan language (*cf.* Johnson & Peeke [241]). Like Warao, it violates Piggott's predictions as shown in (18a.) and (18c.). Since Secoya, as opposed to Warao, exhibits nasal sharing (*cf.* (15)) between G-expressions (w, j, h) in onset position and following vowels, there are no examples as in (18b.):

[26]For an introduction to the theory of phonological derivations and to the phonology-morphology interface, *cf.* Kaye [259].

(19) *More problems for Piggott: Secoya (type A)*

a. mẽã́ ‘variety of ant’
   w̃ẽ́w̃ẽ́ ‘variety of tree’
   j̃ẽ́j̃ẽ́ ‘arm band’

b. rójé ‘variety of fish’
   jáhí ‘sweet potato’

c. -hi ~ -h̃ĩ ‘*3sg. present progressive masculine*’
   séwóhi ‘he is answering’
   w̃ẽ́h̃ĩ ‘he is wrapping’

d. kúmã́ ‘variety of tree’
   kɨ́mã́ ‘variety of tree’
   ɨ́mɨ̃ ‘man’
   hámṍ ‘armadillo’

e. kɨ̃́ʔh̃ẽ́pɨ ‘variety of fowl’
   tɨ̃ṍko ‘she is weaving’
   nã́ʔso ‘crayfish’

f. ã́õ ‘bread’ (*cf.* 1st two examples in e.)
   sṍkɨ ‘tree’
   sẽ́õpɨ ‘clay pot’

(19a.) shows examples which contain only nasal expressions while (19b.) exhibits completely oral words. The forms of the third person singular present progressive masculine in (19c.) provide evidence for analysing Secoya as language with left-to-right nasal harmony while the forms in (19d.) verify that nasality does not spread leftwards in Secoya. This evidence indicates that in each of the forms in (19) which contain nasality there is only one lexically nasal expression: the leftmost N-expression; the nasality in all expressions to its right is derived.[27] In addition, the data in (19e.) establishes Secoya as a type A language, *i.e.* a language in which T-expressions block nasal harmony. The examples in (19f.) and the first two forms in (19e.) prove Piggott wrong: Even though Secoya is clearly a type A language, there are NVs that cannot be explained as derived due to the lack of an available (left-adjacent) consonantal

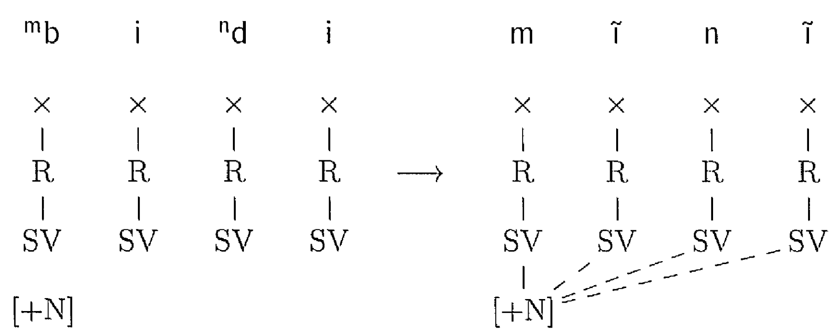
[27] I am not claiming here that in the Secoya lexicon in general, there can only be one nasal expression per domain.

source or trigger.

Having looked at Warao and Secoya, it becomes apparent that Piggott's proposal that the feature [nasal] can be attached to either the Spontaneous Voicing or the Soft Palate node can be proven wrong.

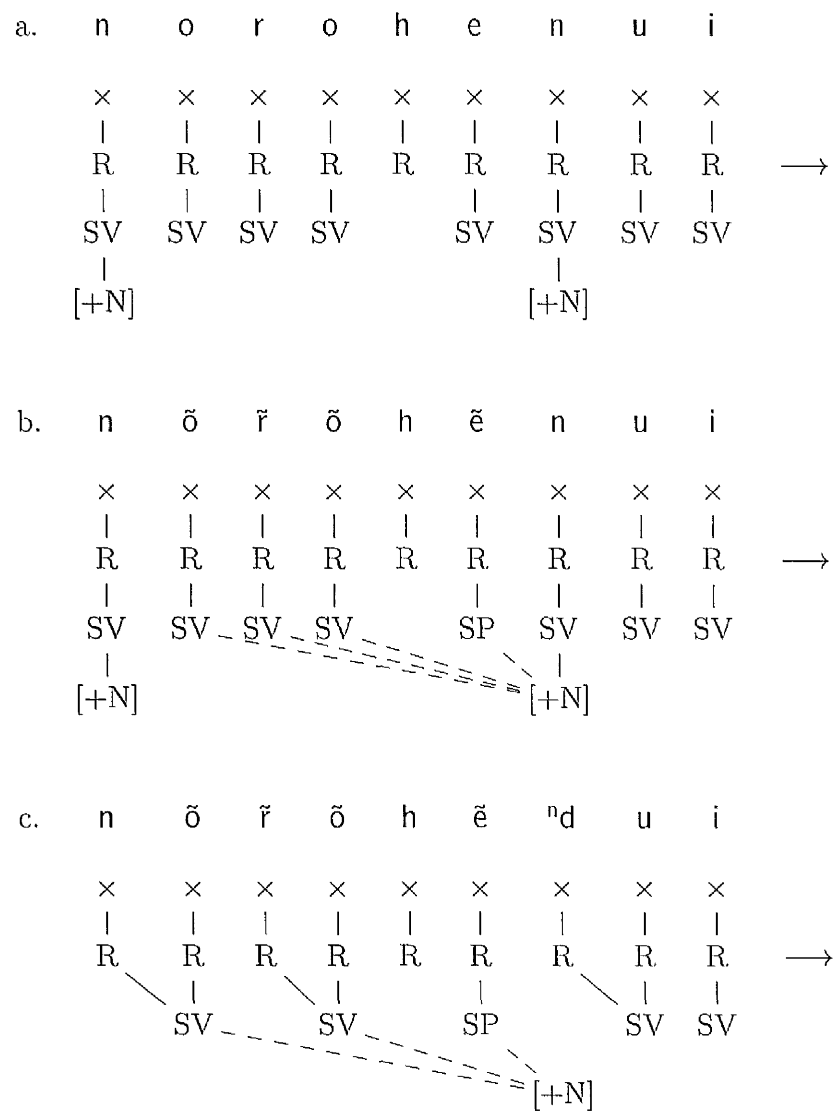
My fifth and final argument against Piggott's account of nasal harmony phenomena is of a typological nature. Piggott [pp. 45–61] discusses two type B languages, SB and Guaraní. Both of these languages display nasal sharing as illustrated in (15) for G- and N-expressions and nasal harmony unblocked by T-expressions; T-expressions may precede either type of nucleus, nasal or oral. As pointed out above, Piggott's explanation of nasal sharing is voice fusion (*cf.* (17)). This claim enables him in both languages to propose only one series of underlying 'segments' for the two observable surface series of N-expressions ('nasal stops') and D-expressions ('(prenasalised) voiced stops'). The point relevant here is that in SB he derives N-expressions from D-expressions while in Guaraní D-expressions are derived from N-expressions:

(20) *SB* mĩ́nĩ 'bird'[28]



[28](20) is taken from Piggott [369, p. 52]. The SB data is again from Smith & Smith [450]. Note that Piggott does not specify pitch for SB.

(21) *Guaraní* nõr̃õhẽ^{n}du̲i 'I don't hear you'[29]



According to Piggott [p. 58], the application of the rule of voice fusion (*cf.* (17)) to the representation of nõr̃õhẽ^{n}du̲i in (21b.) results in the final representation in (21c.). Obviously the D-expression in -hẽ^{n}du̲i is derived from an underlying N-expression; in SB, on the other hand, N-expressions are derived from D-expressions. It is important here to understand that either language has one series of T-expressions and only one series of underlying N- or D-expressions. Piggott's analysis can therefore

[29](nõ-r̃õ-hẽ^{n}du̲-i = not - I (subject):you (sg.) (object) - hear - *negation*; underscoring marks stress). (21a.) and (21b.) are from Piggott [369, p. 59], (21c.) is from [p. 60]. The Guaraní word used can be found in Rivas [412]. Unfortunately, Piggott's view on Guaraní nasal harmony does not take stress into account; for a discussion of the relevance of stress for an analysis of Guaraní, *cf.* Goldsmith [177, 178].

only be maintained if there is independent evidence for the typological categorisation implicit in his account. So, if his analysis is worth its salt then a cross-linguistic survey of languages *without* nasal harmony but with one series of 'voiceless stops' and only one series of either voiced stops *or* nasal stops should demonstrate two types of languages: Firstly, those with an underlying series of 'nasal stops' and no series of underlying 'voiced stops' (prenasalised or not), and secondly, those with an underlying series of (maybe prenasalised) voiced stops and no series of underlying nasal stops. However, "Every language has at least one PNC [primary NC] in its inventory" (Ferguson [158]) and such a generalisation cannot be made about voiced stops. In other words, an underlying series of voiced stops implies a series of nasal stops but not *vice versa.* This means that it is implicit in Piggott's view that languages which do not display nasal harmony cannot have a series of voiced stops without having a series of nasal stops; languages with nasal harmony as analysed by him, on the other hand, might have a series of voiced stops without a nasal series. It can therefore be said that Piggott's account is totally contrary to typological evidence.

I have to add here that there are a few other phonologists who claim that they have found a language without a series of underlying nasal stops (*e.g.* Schachter & Fromkin [430] and Hyman [233]). Let me illustrate this with Schachter & Fromkin's analysis of Akan. They provide the following rule to account for Akan "Regressive Non-Vowel Nasalization" [430, p. 72]:

(22) *Regressive non-vowel nasalisation in Akan*

$$\begin{bmatrix} -\text{vocalic} \end{bmatrix} \longrightarrow \begin{bmatrix} +\text{nasal} \end{bmatrix} \;/\; \left\{ \begin{matrix} \begin{bmatrix} \text{\_\_\_} \\ +\text{consonantal} \\ +\text{voiced} \end{bmatrix} \\ \begin{bmatrix} \text{\_\_\_} \\ -\text{consonantal} \end{bmatrix} \end{matrix} \right\} \;/\text{\_\_\_} \begin{bmatrix} +\text{vocalic} \\ +\text{nasal} \end{bmatrix}$$

In Schachter & Fromkin's own words, the rule in (22) expresses

> "that a [−Vocalic] segment becomes [+Nasal] if the segment is marked either as [+Consonantal, +Voiced] or as [−Consonantal], and if, further, it immediately precedes a [+Vocalic, +Nasal] segment." [p. 72]

According to a phoneme table provided by Schachter & Fromkin [p. 41], the Akan segments underlyingly specified as [+consonantal, +voiced] are b g d, the ones specified as [−consonantal] are j h w; vowels are only redundantly and thus not underlyingly [−consonantal]. In short, the rule in (22) states that underlying b d g j w h become nasalised, *i.e.* m n ŋ j̃ w̃ h̃ (respectively), when directly left-adjacent to a NV.[30] The following examples are given [*ibid.*]:[31]

(23) *Nasal sharing in Akan (as analysed by Schachter & Fromkin [430])*

a. *Underlying form*	b. *After rule in* (22)	
bã	mã	'give'
dã	nã	'and'
jã	ɲã (or j̃ã)	'receive'
wãdĩ	ŋ͡w̃ãnĩ (or w̃ãnĩ)	'scrape'
hũ	h̃ũ	'fear'

In this analysis of Akan, there are no underlying NCs and all audible NCs are regarded as surface forms, *i.e.* allophones, generated via a rule from underlying voiced stops. As pointed out above, this is contrary to typological evidence. This problem is worsened by the fact that, additionally to the rule in (22), Schachter & Fromkin have to stipulate yet another rule which also derives surface NCs from underlying voiced stops. This second rule derives NCs either from word-internal voiced stops which precede consonants other than j w h or from word-final ones. For example, surface [dum] is generated from underlying /dub/ 'extinguish', [tumpãŋ] from /tubpãg/ 'bottle' [p. 73]. This, again, is contrary to typological evidence: Universally, sequences of 'NC plus oral stop' are the most common consonant sequences.[32] This means that in languages without nasal sharing the existence of such sequences does not imply the existence of any other type of consonant clusters. It therefore appears unmotivated that in Schachter & Fromkin's analysis of Akan, a language with nasal sharing, there are no such usually common underlying sequences like /ṁb/ or /mp/. Instead Schachter & Fromkin assume underlying clusters of 'voiced stop plus other oral stop', like /bp/ or /gk/ (surface forms [mp] or [ŋk], respectively), which are excluded in virtually all Indo-european languages, even though the latter display far fewer phonotactic restrictions on consonant sequences than Akan (or any of the languages discussed in this section).

[30] Schachter & Fromkin [p. 72] give ɲ ŋ͡w̃ as additional variants of j̃ w̃, respectively.

[31] Schachter & Fromkin put underlying representations (23a.) between 'phoneme' slashes ('/ /') and surface forms (23b.) between square brackets ('[ ]').

[32] Herbert [213, pp. 54ff.] discusses the highly frequent co-occurrence of open syllable structure and (word-internal) prenasalised stops.

Another linguist who claims to have found languages without a series of lexical N-expressions, is Le Saout [290]. Based on research on the phonologies of a number of Kwa and Mande languages,[33] Le Saout states:[34]

> "Le système phonologique des langues que nous avons examinées serait ainsi caractérisé par:
> —l'absence d'unités consonantiques nasales (PNC);
> —la présence d'unités vocaliques orales et nasales" [290, p. 204].

As in the case of Schachter & Fromkin's analysis of Akan, all of the languages Le Saout's discusses do not only exibit no opposition between N- and D-expressions (at least for some 'places of articulation') while they do display an opposition between nasal and oral vowels, they also aways have underlying D-expressions which are, in certain contexts, realised as nasal stops.

It appears that those phonologists who do not assume an underlying series of NCs *always* propose an underlying series of voiced or prenasalised voiced stops which is in variation with a series of corresponding NCs. Note that Herbert [213, pp. 6-51] shows that a series of prenasalised voiced stops in languages without a series of voiced stops can usually be analysed as series of voiced stops. Consequently, it is not relevant whether or not the underlying voiced stops proposed are prenasalised. Also, languages supposedly without underlying nasal consonants obligatorily exhibit nasal harmony or nasal sharing resulting in the underlying series of voiced stops being in complementary distribution with the nasal series. This means that Schachter & Fromkin, Hyman, Le Saout and like-minded phonologists ignore typological evidence just as much as Piggott does: For them, each language must have an underlying series of NCs unless the language has some kind of nasal assimilation process. Unfortunately, it remains completely unclear why only (some) languages with nasal harmony should not have an underlying series of nasal stops. This is evidence against the proposal of underlying voiced stops but not of nasal stops for any language independently of whether or not that language displays some kind of nasal assimilation

[33]Research mainly by Ansre [11, 12], Bearth [30], Bearth & Zemp [31], Carrel [78], Courtenay [117], Doneux [141], Fresco [169], Hockett [220], Ladefoged [281], Le Saout [288, 289], Prost [400], Siertsema [447], Stewart [459], Ward [486] and Welmers [489, 490]. Further discussion of Le Saout [290] can be found in Bentinck [42].

[34]'PNC' stands for 'primary nasal consonant' and is a term borrowed from Ferguson [158], *cf.* above. Translation (mine): 'The phonological system of the languages which we have examined is to be characterised by: —the absence of nasal consonantal units (PNC); —the presence of oral and nasal vocalic units'.

and of whether this assimilation is achieved via nasal spreading through a whole word, nasal sharing or both.

Having looked at Piggott's analyses of SB and Guaraní, it is now clear that Piggott does not take the typological evidence presented into account. Since in order to make his proposals work, he has to analyse SB as a language without a series of nasal stops, I conclude that even if there were no other counter-arguments, Piggott's analysis of nasal harmony phenomena is flawed.

Let me sum up my critique of Piggott's account of nasal harmony phenomena:

1. Piggott's analysis is within the framework of feature geometry.

2. Piggott's 'revolutionary idea' to assume that the feature [nasal] can be dependent on either the Soft Palate node or the Spontaneous Voicing node cannot be motivated phonetically—which it should be in feature geometry—and this alteration increases the degree of stipulation inherent in his approach.

3. Piggott cannot motivate independently which consonants have to be specified for Spontaneous Voicing. In other words, within Piggott's phonetically motivated analysis, the assumption of voice fusion remains unmotivated by phonetics and is therefore arbitrary.

4. Piggott cannot account for data found in many type A languages, *i.e.* one of the types of nasal harmony languages predicted by him. Specifically, the prediction that in type A languages only consonants may underlyingly be specified for nasality and trigger nasal spreading can be proven wrong.

5. Contrary to strong typological evidence, Piggott has to propose an analysis for a certain subtype of type B languages (exemplified by SB) in which for him there are no underlying NCs.

As demonstrated, an investigation of the theoretical basis of feature geometry and its practical consequences in an analysis of nasal harmony phenomena by Piggott makes it evident that feature geometry as phonological framework is in many ways arbitrary and cannot add anything to an explanation of phonological 'nasality'. It can thus be said that Piggott's attempt at providing an account for nasal harmony fails due to the detrimental assumption of the fundamental correctness of feature geometry and due to a number of additional proposals which ultimately increase the degree of stipulation inherent in his approach, can be proven wrong or are contrary to typological evidence.[35]

Conclusion

In the first two chapters of this theses, I have shown that there is no evidence in favour of any articulatory or acoustic version of the PH and that the phonological mainstream therefore works within theories which are fundamentally flawed. Unfortunately, this subject is usually ignored by the supporters of the PH. To back these claims up, I have mainly looked at phenomena involving 'nasality'.

This means, firstly, that all analyses which are given within frameworks that presume the PH and which refer in any way to the internal representations of phonological expressions are fundamentally flawed.

Moreover, since most mainstream frameworks refer to nasality in form of a feature, I have discussed various methods employed by phonetically based feature theories to avoid the extensive overgeneration inherent in such systems and have looked at a number of modern incarnations of the PH including popular feature theories like underspecification theory and feature geometry. This argumentation has provided abundant evidence for my claim that such approaches must incorporate the PH in a way that ensures that it is not falsifiable. A further important finding is that the commonly assumed feature [±nasal] or the presumption of underspecified nasality may well be phonetically but is sadly not empirically grounded.

[35] In [371], Piggott rejects the PH and subscribes to a cognitive explanation of the phonology of nasality (the 'Phonological Hypothesis').

Chapter 3

The Height Myth

Introduction

In the linguistic literature, it has, as I will demonstrate, become an accepted point of view that there is a phonetically motivated synchronic correlation between nasality and vowel height and similarly, even though somewhat less prominently, between nasality and consonantal place of articulation. A related and widely discussed claim is that due to the inherent affinity between phonetically defined nasality and quality, low vowels nasalise diachronically earlier but denasalise later than non-low ones. It is these two claims regarding nasalisation and denasalisation, *i.e.* the assumed correlations between phonetically motivated phonological 'nasality' and 'height', on the one hand, and between phonological-phonetic nasality and height-related timing of historical (de)nasalisation, on the other, which I will refer to as the 'Height Myth' (henceforth 'HM'). Having shown in the previous chapters that the PH constantly fails by making wrong predictions or by being unfalsifiable, it is now important to evaluate whether or not the HM, *i.e.* another wide-spread phonetically motivated assumption regarding nasality, holds true. In this chapter, I refute the relevance of the HM and argue that phonological 'nasality' is a phenomenon totally independent of height.

To demonstrate this I first look at historical evidence which has been put forward to show that vowel nasalisation and denasalisation are influenced by the quality, and in particular the height, of the phonological expressions involved (*cf.* section 3.1). More specifically, I discuss the development (3.1.1.1) and denasalisation (3.1.1.2) of French NVs (3.1.1), the development of NVs in Chinese (3.1.2) and outline the arguments for Hombert's [222, 223] claim that, when preceding a NC, Proto-Bantu

°a, °e, °o[1] are more likely to become distinctively nasalised in the (modern) Teke language group than °i, °u. Furthermore, I provide data illustrating a case of preferred nasalisation of a in certain contexts in a number of Romagnol dialects. For each of these cases, I argue that the available data does in no way contain evidence in favour of a (phonetic) nasality-height correlation or the proposal of height-related historical (de)nasalisation.

The second section of this chapter is concerned with synchronic evidence for the HM. I will discuss claims that nasalisation in vowels has an influence on their height and and that this can be motivated phonetically (section 3.2.1). After that, I will look at the dubious nature of many of the data often referred to in support of a synchronic version of the HM. This involves a rejection of the (usually tacit) assumption that it is part of the phonology to find analyses of morphologically related forms in which such forms are derived synchronically—and phonetically!—from a common source (section 3.2.2) or in which they are etymologically related (3.2.3).

3.1 Historical evidence

Let us now look at the historical evidence that has been put forward in favour of the HM.

3.1.1 French: development and denasalisation of nasal vowels

To start the historical argumentation, I will discuss the development and denasalisation of French NVs. Both of these processes (supposedly) provide an important part of the historical evidence for the HM.

3.1.1.1 The development of French nasal vowels

A number of researchers, most prominently Pope [390, 391] and Haden & Bell [194], have worked on the emergence of NVs in French and have come to the conclusion that this development is tightly connected to the quality of a vowel before its (diachronical) nasalisation. More specifically, such linguists claim that low vowels were nasalised *earlier* than high vowels. Within this section, I will refer to this view as the 'standard' explanation or theory.

[1]Note that in order to differentiate ill-formed from reconstructed material, I will employ a raised asterisk '*' to denote ill-formedness and a raised circle '°' for reconstructed forms.

Since most of the arguments for this proposal are based on assonance patterns, let me first give a brief introduction to this type of rhyme. Assonating rhyme in French can be found from the end of the 9th century, *e.g.* in the *Séquence de Sainte Eulalie*, the earliest known French poem, or in the *Chanson de Roland* (12th century). Assonance has in common with 'perfect' rhyme, *i.e.* the kind of rhyme that we are all familiar with, that in order for two domains to assonate their rightmost stressed vowels must be identical. However, for assonance, unlike for perfect rhyme, two domains need not be (nearly or completely) identical with respect to the phonetic material following the last stressed vowel. For example, in English, ten and ton neither rhyme perfectly nor assonate, ten and when rhyme perfectly *and* assonate; ten and tell, on the other hand, do not rhyme perfectly but do assonate.

Let me provide a few verses from the *Chanson de Roland*:

(1)

a. Sur un perrun de marbre bloi se culchet; *(from verse ii)*
Envirun lui plus de vint milie humes.
Il en apelet e ses dux e ses cuntes :

b. Eneiuns i les filz de noz muillers : *(from verse iii)*
Par num d'ocire i enveierai le men.
Asez est melz qu'il i perdent lé chefs

c. En quel mesure en purrai estre fiz? *(from verse x)*
—Vos par hostages", ço dist li Sarrazins,
"Dunt vos avrez u dis, u quinze, u vint.

d. Turpins de Reins en est levet del renc *(from verse xix)*
E dist al rei : "Laisez ester voz Francs!
En cest païs avez estet set anz :

The verses in (1a.) show that the sequences ⟨ulch⟩, ⟨um⟩, ⟨unt⟩ assonate. This is, in general, taken to be evidence for the assumption that u preceding a NC (as in ⟨um⟩ or ⟨unt⟩) is not nasalised (yet); this is why it can still assonate with (oral) u (as in ⟨ulch⟩). Similarly, eN-sequences (as in ⟨men⟩) still assonate with eC-sequences (as in ⟨ers⟩ and ⟨efs⟩, *cf.* 1b.), iN-sequences (⟨inz⟩ and ⟨int⟩) with iC-sequences (⟨iz⟩, *cf.* 1c.). Therefore, so the standard theory goes, e and i in eN- or iN-sequences, respectively, are still oral vowels. Assuming that the verses in (1d.) are typical for the *Chanson de Roland*—and they are, I have checked!—it can be said that some eN-sequences (those containing e_2 as in ⟨renc⟩) assonate with aN-sequences, while other eN-sequences (e_1), *e.g.* those like ⟨en⟩ in (1b.), assonate with (yet oral) eC-sequences. In this view, the reason why e_2-sequences appear to have merged with aN-sequences is that both cluster types contain one and the same *nasalised* vowel. This claim is supported by the fact that verses containing aC-sequences (almost) never assonate with aN- or e_2-sequences and *vice versa*, which means that there is independent evidence to say that aN-sequences contain a nasalised a, *i.e.* ã or ɑ̃, which cannot assonate with oral a anymore. Since a in aN-sequences and e_2 in e_2N-sequences are 'clearly' nasalised while e_1, i and u are not, this is (supposedly) a strong argument for the standard theory, *i.e.* the claim that low vowels in French were nasalised before high ones.

Having seen how assonance works and how versification might provide evidence for the standard view on nasality-height correlation, let us now take a closer look at the quality of the evidence extracted from assonating poetry. One might expect that there was a period in French poetry when all possible sequences '(still oral) vowel followed by OC' assonated with sequences '(still oral) identical vowel followed by NC', *i.e.* when any V_iC-sequence assonated with any V_iN-sequence. A major problem of any discussion of evidence provided by assonating poetry is that there are apparent gaps: some assonances are not attested. That is to say that from the earliest sources on, some VC-sequences never assonate with their corresponding VN-sequences. This is further complicated by the fact that not all scholars agree on which assonances are or are not attested and on when certain vowels stopped assonating (*e.g.* Entenman [153, p. 306] regarding Pope's [390, p. 169] views).

Neglecting these problems, let me now explain the development of NVs as Haden & Bell [*ibid.*] see it. This will make clear that the standard view is not conclusive. I will begin by providing a summary of assonances as assumed by Entenman, *i.e.* a summary containing the above mentioned 'gaps'. Note that Entenman speaks in this context about a "hypothetical period" in French poetry because his summary is based on an extensive study of other scholars' research who, partly in disagreement with him, believe that some of the non-assonating pairs in (2) did assonate. For convenience's sake, let us agree with Entenman's [153, p. 306, footnote 2] findings: so using Bell [37] as guide and contrary to Pope [390, p. 169], it can be said that in the *Séquence de Sainte Eulalie* (9th century), *La Vie de St. Leger* (10th century) and in *La Vie de St. Alexis* (11th century), there are no examples of assonances for pairs marked by a raised dagger in (2). Unfortunately, this discussion is made even more complex by evidence that suggests that, as Entenman puts it, "the data are probably not as neat [!] as pictured here" [153, p. 306]: For example, Jenkins [237], altered the text of the *Chanson de Roland* to eliminate assonances which had supposedly died out two centuries earlier, *e.g.* aC assonating with aN. However, Entenman's [153, p. 263] 'hypothetical' state of affairs is as follows:

(2)

iC/iN	uC/uN	ieC/ieN
†eC/eN	oC/oN	†eiC/eiN
†aC/aN		†aiC/aiN

Over the next centuries, assonance between more and more V_iC-V_iN pairs ceased. Entenman [153, p. 265] tries to provide a summary of Haden & Bell's [194] research regarding this. He admits that he is "not certain of the phonetic values they [Haden & Bell] would assign in every case" [*ibid.*] in this summary because they "don't actually present their analysis in quite this explicit a manner" [*ibid.*]. Furthermore, Entenman [pp. 265f., 306ff.] questions some of the assonances in Haden & Bell's analysis summed up in (3). However, to illustrate Haden & Bell's argumentation in favour of a nasality-lowness correlation in French, let me provide Entenman's summary [*ibid.*] of their findings:

(3)

century:	9-10th		11-12th		13th	
	/_C	/_N	/_C	/_N	/_C	/_N
⟨e⟩	e	ẽ	e	æ̃	ɛ	ɑ̃
⟨a⟩	a	ã	a	æ̃	a	ɑ̃
⟨ai⟩	ai	ai	ai?	æ̃i	ɛ	ɛ̃
⟨ei⟩	ei	ei	ei	ei	ɛ	ɛ̃

Vowels within one and the same box assonate. Note that in the column '13th century, /_ C', *i.e.* the second column from the right, ⟨e⟩ [ɛ], ⟨ai⟩ [ɛ] and ⟨ei⟩ [ɛ] assonate. ⟨e⟩ in (3) can, in my view, only refer to what I have labelled 'e_2' above. Also, I will neglect the implications of Haden & Bell's proposal that vowels which are supposedly phonetically different, *e.g.* [æ̃] and [æ̃i], can nevertheless assonate. However, for Haden & Bell, ⟨e⟩ (e_2) when preceding N starts out as [ẽ] in the 9th and 10th century, changes to [æ̃] in the 11th and 12th century and ends up as [ɑ̃] in the 13th century. Similarly, ⟨a⟩ in the same context changes from [ã] to [æ̃] to [ɑ̃] over the same period of time. In addition, not only the quality of nasalised but also of presumably oral vowels undergoes change: Haden & Bell assume that ⟨ei⟩ in oral contexts is realised as [ei] from the 9th to the 12th century but then, in the 13th century, as [ɛ].

It is important here to draw one's attention to a remarkable observation that we have just made: Haden & Bell, like Pope [390, 391], do not only propose diachronical nasalisation of vowels in VN-sequences (starting with low vowels), they also have to assume *simultaneous changes* in the *quality* of these 'nasal' vowels (*cf.* Entenman [153, p. 264]). And it is this obligatory 'coincidence' that makes the standard view inconclusive. The crucial point is that

> "... beginning with the 11th and 12th centuries, differences in vowel quality alone are sufficient to account for many of these changes in assonance." [153, p. 266]

For example, the non-occurrence of assonance between a in aC- and aN-sequences, ai in aiC- and aiN-sequences and ei in eiC- and eiN-sequences does not have to be due to nasalisation but could also be due to changes in vowel quality. Since an explanation other than the standard one is possible, it is not sufficient for the standard view to have some kind of story for the cessation of assonance. So it is the supporters of the standard theory of French diachronical nasalisation who have to provide conclusive evidence in favour of their claim that there is some kind of universal correlation between nasality and vowel height. This means that if their explanation is based on gradual change of assonance patterns, they have to show that this cessation could *only* be due to nasalisation. If they cannot provide this evidence—and it seems that they cannot—, they have no argument and any continuation of their claim becomes a matter of faith.

Let me also point out here that it would in my view not save the standard explanation to invoke 'phonetic evidence' for a 'universal tendency' of nasality to correlate with some other phonetically defined property like height. Firstly, if something 'universal' is merely a tendency it is precisely *not* universal; and since there are numerous languages which do not display any correlation between nasality and height, *e.g.* Tupí-languages like Guaraní [187, 310, 412] or Portuguese [77], the standard view could not refer to anything more testable than a tendency. Secondly, as I have shown in the previous chapter, there is no 'phonetic evidence' for phonology. Any so-called phonetic evidence always turns out to be unfalsifiable. Not surprisingly, the term 'universal tendency' exemplifies this problem with science a phonetically based phonologist has: The term 'tendency' is so vague that any case which does not comply with this tendency justifies calling it a tendency. All counter-examples to a tendency are by definition no counter-examples anymore. This makes such a tendency an unfalsifiable assumption which one can uphold without having to question it. So, I can maintain that there is no conclusive evidence for the proposal that, historically, low vowels in French got nasalised before non-low ones.

Additionally, Entenman discusses a whole series of further arguments against the standard theory of the development of French NVs. Let me briefly sum them up: He can show that, opposed to the traditional view, oral and nasal vowels *can* assonate in French. He [p. 270] points to Pope who, probably unwittingly, admits that uN, which in Pope's opinion had already become nasalised by the early 12th century, can nevertheless still assonate with uC at that time. This points to an inconsistence inherent in the standard view: On the one hand nasalisation is assumed to be the cause for the observable change in assonance patterns; but sometimes, a vowel presumed to be nasalised can evidently assonate with its oral counterpart. Entenman admits, however, that in French the evidence for nasal-oral assonance is relatively weak. He therefore looks at Portuguese assonance and shows that two vowels do assonate in Portuguese independently of whether any one of them is nasalised as long as their respective (nasality-independent) qualities are the same [pp. 270–274].[2]

To continue, the standard theory assumes that when different VN-sequences start to rhyme, they do so because their vowels have become nasalised and, due to nasalisation, have merged. For example, since iN begins to rhyme with eiN and aiN in the 15th century (*cf.* Bell [37, pp. 113f.]), Delattre [132] argues that high vowels must have become nasalised by then. However, such a merger does not necessarily have to be caused by nasalisation but could be due to changes in vowel quality. Delattre [132, p. 224] appears to admit this himself, maybe unknowingly:

> "(We must suppose that rimes between oral vowels and their corresponding nasalized vowels did not cease to be possible until the nasalized vowels had undergone a certain degree of modification in vowel quality . . .)" [Entenman's [153, p. 262] translation]

There is further evidence that the standard theory is unlikely: evidence based on orthography. In French (past and present), NVs are not marked by the orthography. Certain conventions have to be learned. For example, words ending orthographically in a (single) NC are, in Modern French, pronounced with a word-final NV (⟨don⟩ [dɔ̃]

[2] In his discussion of Portuguese assonance, Entenman unfortunately refers to 'Cunha (1961)' a great deal, a reference for which there is no entry in Entenman's bibliography.

'gift'). Over the centuries, French writers started to confuse different symbols for NCs. Pope [390, 169] points to an example from 1065: ⟨Dinam⟩ instead of ⟨Dinan⟩ (*cf.* Entenman [153, 278]). Pope takes orthographic confusions of this kind as evidence in favour of a merger of word-final NCs. This merger is in her opinion due to strong nasalisation of the preceding vowel, resulting in a dental NC, which, according to Ruhlen [417], turned into a velar nasal.[3] However, it is not possible to find out whether these changes in spelling are due to merger or loss of final NCs.

For argument's sake, let us agree with Pope's and Ruhlen's assumption that such orthographic confusions were caused by nasalisation of the preceding vowel. So a supporter of the standard view on French nasalisation should expect such orthographic changes to have started with cases where the final NC follows the orthographic representation of a low vowel. However,

> "... all that is important to the present discussion is that there seems to be no evidence that N-loss or neutralization occurred only after low vowels. ... such evidence cannot be used to support the hypothesis of low vowel nasalization." (Entenman [153, p. 282])

To be clear here, it is not only the case that there is no evidence in favour of the standard view; more importantly, if the standard theory were correct in proposing that low vowels became nasalised first, one would expect that the changes in spelling started with low vowels and proceeded to the high vowels. In other words, the indiscriminatory manner in which the orthographic confusion of final NCs occurred is strong evidence *against* the standard explanation and points to equally indiscriminatory nasalisation of French vowels, *i.e.* simultaneous nasalisation of all vowels in certain context, independently of vowel quality.

Let me now summarise the problems of and arguments against the standard view on French nasalisation:[4]

[3] Entenman [pp. 280f.] discusses briefly the in-between steps this merger took, *e.g.* whether the palatal nasal and the labial nasal became dental first and then the dental nasal changed into a velar one, or whether all NCs moved 'backwards' (articulatorily), in which case the palatal nasal would have changed into a velar one without becoming dental first.

[4] In this section, I have neglected evidence based on the use of double consonants [153, pp. 282ff.], the writing system invented by Guiot, a scribe of the 13th century, [153, pp. 284ff.], and statements about the pronunciation of French by the Englishman Jehan Palsgrave (1530) [153, pp. 286f.]. Regarding Guiot and Palsgrave, *cf.* [32] and Geschiere [172], respectively. Note that the article by Geschiere from 1968 appears as 'Geschière (1970)' in Entenman's bibliography [153, p. 326]. To give the reader an idea what kind of statements on the part of Palsgrave such evidence is based on, here is a sample, regarding the two nasal pronunciations of the vowel *E* that Palsgrave recognises: "Sometyme they sounde hym lyke an *a* and a lyttell in the noose, and sometyme almost lyke an *o* and very moche in

1. The earliest known French poems do not display all theoretically possible assonances between V_iC- and V_iN-sequences. The changes in assonance patterns must have started before the earliest sources.

2. Not all scholars agree on which assonances are or are not attested. Entenman, for example, disagrees with Pope.

3. Some editors of French poetry, *e.g.* Jenkins [237], have altered the sources to make them fit their views on 'authentic' assonance patterns.

4. Pope [390], one of the most prominent scholars of the 'standard' type, assumes that uC still assonates with already nasalised uN in the early 12th century, even though nasalisation is presumed to prevent a V_iN-sequence from assonating with its corresponding V_iC-sequence. It appears that an oral vowel *can* assonate with a NV as long as it is identical in quality—*not* nasalisation.

5. A strong argument against the standard view is that it is inherently inconclusive: The attested development in assonance patterns could not only have been caused by the onset of nasalisation but also by simultaneous changes in vowel quality, changes which supporters of the standard explanation *have to* assume for V in VC- and VN-sequences.

6. Finally, changes in the spelling of final NCs point to a merger of such nasal stops and the nasalisation of a vowel preceding a final nasal. This merger apparently occurred independently of the quality of the vowels involved. This is strong evidence against the standard theory's claim that nasalisation affected low vowels first.

I conclude that, as prominent as the standard view on French nasalisation might be, there is no substance to it. The history of the nasalisation of French vowels does not provide evidence for the claim that low vowels became nasalised earlier than high ones and does thus not support the proposal of a correlation between nasality and height.

3.1.1.2 The denasalisation of French nasal vowels

As for section 3.1.1.1, Entenman has already done most of the relevant research [153, pp. 290–305]. In the following, I will therefore mainly sum up his findings, which

the noose" [170, p. 3], also [172, p. 186]. I agree with Entenman that none of these cases appear to be conclusive.

show that, contrary to the standard view, conditioned NVs do not denasalise starting with high vowels ("morphological" denasalisation [153, p. 291]). Entenman does however find some evidence which suggests that, in line with the standard theory, the denasalisation of unconditioned NVs does begin with the high vowels of a language ("phonetic" denasalisation [*ibid.*]). This last result of Entenman's research will be the only one that I will ultimately refute. I will demonstrate that Entenman, who in most of his work is admirably accurate[5] and not easily taken in by some mainstream analysis, was let astray by the tempting assumption that phonetics is relevant to phonology.

I will start with 'conditioned' denasalisation, *i.e.* denasalisation conditioned by vowel height in the historical development of French. Standard theorists, namely Pope [390, 391] and Ruhlen [417], claim that, while nasalisation starts with low vowels and proceeds to the high vowels of a system, denasalisation works the other way round, so high vowels denasalise first, low vowels last. The reason for this order is for both Pope and Ruhlen a phonetic one. Pope sees the cause for denasalisation in ease (or rather, unease) of articulation. Pope thinks that it is more difficult to pronounce high NVs than low NVs. Similarly, Ruhlen considers the natural order of things, *i.e.* markedness conventions, to be the best explanation. Because it is not natural for NVs to be high, they are the most eager vowels to revert to orality. It is therefore no surprise to Pope or Ruhlen to find that high vowels denasalised in French earlier than low vowels. Typically, the evidence for saying that this is how French actually developed is rather weak. To demonstrate this, let me sum up Entenman's arguments against the standard view. Note that in his discussion, Entenman only uses a and i as examples for low or high vowels (respectively).

The strongest part of Pope's evidence is that there are morphologically related forms in Modern French which can only be explained by proposing that, historically, NV lowering occurred *after* denasalisation (4a.). Consider the following example:[6]

[5]This does explicitly not refer to some of his bibliographical references.

[6]The example in (4a.) is similar to the one in Entenman [153, p. 295], *cf.* Pope's [391, pp. 174, 307] comments regarding ⟨fin, fine⟩. The empty set symbol '∅' stands for 'not applicable'.

(4) *The development of Modern French* ⟨fin, fine⟩ *'fine, delicate'*

a. The right result

masculine	*feminine*	
fĩ	fĩnə	original forms
∅	finə	denasalisation ($\tilde{V} \Rightarrow V/\_C_N V$)
fɛ̃	∅	NV lowering
fɛ̃	fin(ə)	Modern French forms

b. The wrong result

masculine	*feminine*	
fĩ	fĩnə	original forms
fɛ̃	*fɛ̃nə	NV lowering
∅	*fɛnə	denasalisation ($\tilde{V} \Rightarrow V/\_C_N V$)
fɛ̃	*fɛn(ə)	Modern French forms

In cases such as in (4), it is possible to say that lowering indicates that denasalisation must already have taken place. Based on data of this kind or on the observations of 16th century grammarians like Bovelles (1533, quoted from Thurot [475]) and Tabourot (1587) [471], Pope believes that, in educated Parisian French, ĩ denasalised in the 15th century (*cf.* [391, p. 175]); the denasalisation of ã, on the other hand, she proposes for the later 16th century ([391, pp. 172f.]).

Entenman [pp. 296ff.] points to two problems with Pope's argumentation. Firstly, the standard view on historical ordering "may well have been restricted to educated Parisian speech" [p. 297]; so, according to Pope's data, northern French varieties lowered NVs much later than southern varieties while southern dialects lowered NVs *before* denasalising them. Secondly, it appears, according to Pope's own evidence but contrary to her conclusions, that the denasalisation of ĩ occurred approximately simultaneously with the denasalisation of ã. Entenman's argument for this is as follows:

Entenman [pp. 298ff.] shows that the observations of contemporary grammarians on which Pope bases her proposals are not necessarily in favour of her deductions; these observations can also be interpreted in a way such that ⟨in, ain, ein⟩ were still realised as high vowels in the 17th century. This would indicate that they were in a state *before* lowering occurred, *i.e.* when denasalisation had either not started to apply yet or was applying at that time. This period (17th century), though, coincides with the time which Pope assumes for the beginning of the denasalisation process of ã.

In other words, it looks as if denasalisation of ĩ and of ã might well have occurred simultaneously; it is not clear at all that ĩ must have been denasalised before ã started to follow.

To sum up, Pope's claim that the high NV ĩ was lowered in the 16th century is apparently not conclusive. Since lowering presupposes denasalisation, this means that also the evidence for the claim that denasalisation of ĩ must have applied by the end of the 16th century, *i.e.* earlier than ã, is not conclusive.

Let me now briefly discuss Entenman's view that, opposed to "morphological", *i.e.* conditioned, nasalisation as in French, "phonetic", *i.e.* unconditioned, denasalisation might well start with high vowels. Entenman [pp. 304ff.] points to Ruhlen [417] who lists a whole series of languages in which, according to Ruhlen, there is evidence for high vowel denasalisation. Entenman rightly asserts that almost all of theses cases could alternatively be analysed as languages which simply do not have high NVs. The mere fact that a language with NVs does not have high NVs does not presuppose the existence of high NVs in the past which denasalised later on. The languages in question might as well never have had high NVs. For the acquirer of a language L_A with no high NVs, it makes no difference whether or not there ever were high NVs in diachronically earlier versions of L_A.

Of course, typological questions of the following kind still need to be addressed:

1. Why do some languages with NVs not exhibit high NVs?

2. Why are there no languages with NVs which do exhibit high but no low NVs?

3. Why do some languages with NVs which have a tense-lax distinction for oral vowels not exhibit this opposition for NVs, typically i u e ɛ o ɔ a but ĩ ũ ẽ *ɛ̃ õ *ɔ̃ ã?[7]

I will try to provide answers to such questions in chapters 4 and 6 (pp. 229ff.). However, Entenman can find "no reason to question the standard theory, in so far as 'phonetic' denasalization is concerned" [p. 304]. Since this means that he needs to differentiate 'morphological' from 'phonetic' nasalisation, Entenman makes two attempts at finding justification for it.

His first attempt is to try to establish a qualitative difference between nasal and oral vowels: In obscure and almost mystical language, he proposes a vaguely defined distinction between vowels which have been built with more (NVs) or less (OVs)

[7]Or, for the nasal series, ĩ ũ *ẽ ɛ̃ *õ ɔ̃ ã.

creative effort. He does not clarify this point further. So he seems to think that nasalisation is a case where

> "a whole new type of sound must come into being. The prosodic system of a language must change ... Notice that denasalization does not involve such a change ... it makes no sense to speak of the creation of oral vowels ... it is certainly possible that phonetic factors determine the order of denasalization, perhaps from high to low vowels." [153, pp. 290f.]

Apparently, Entenman's decision that nasalisation creates NVs out of oral vowels but denasalisation does not create oral vowels out of NVs is completely arbitrary. Nevertheless he considers it reasonable to assume that the order in which NVs are denasalised is dependent on the phonetically defined notion 'height', while the order in which oral vowels are nasalised is not. Also note that Entenman calls denasalisation conditioned by vowel height 'conditioned', denasalisation unconditioned in this way 'unconditioned'. This is to say that for him, a phonetically defined concept like vowel height *may* or *may not* be the conditioning factor of a nasalisation phenomenon and *may* or *may not* trigger a historical change. As always, phonetics is only relevant when it is. The assumption of some kind of general relevance of phonetics for phonology implies the repeated non-application of this assumption. Due to the arbitrariness of Entenman's distinction and the imprecise nature of its predictions, I will disregard this view.

To find further justification for his claim that unconditioned denasalisation is essentially different from conditioned denasalisation, Entenman invokes the unmarkedness of oral vowels by referring to Ruhlen (with whom he normally disagrees). Ruhlen is under the impression that it is "no fundamental mystery" [417, p. 15] that NVs get denasalised. Since, in Ruhlen's opinion, NVs are "certainly" [*ibid.*] more complex than oral vowels, their disappearance is simply a case of NVs changing back "to their normal unmarked state" [*ibid.*]. Unfortunately, this is a typical example of phonetics not making any contribution to the scientific explanation of a linguistic phenomenon. I agree, all languages have oral vowels, while only some have nasal ones. However, the terms 'normal' and 'unmarked' have no predictive power and are here, as elsewhere, used to justify an unfalsifiable assumption: If it is so 'normal' to be oral, how come that an abundant number of languages—like French, Portuguese, Polish, hundreds of languages of the Americas—can have NVs which refuse to exhibit even the slightest tendency to revert back to nature, *i.e.* orality? Also, in what way then was the development of NVs in French natural? To the phonetically minded phonologists of naturalness, naturally scientific questions of this kind do not even make sense, since unmarkedness and naturalness are to them a matter of degree to such an extent that almost any number of counterexamples can be disregarded by them without this affecting their 'theory' in any way. Given the obvious application of the strategies of denial (*cf.* section 1.3.1) and of flexibility of applicability (section 1.3.2), I will not take this argument seriously. It appears that Entenman, misled by contemporary theory, assumes that phonetics is relevant to phonology and in doing so, becomes unable to explain the apparent differences between i and u, on the one hand, and e, o and a, on the other. More importantly, there is no evidence that these differences are due to height or any other phonetically defined notion. On the contrary, the HM, a version of the PH, can only be upheld by conveniently neglecting its predictions when, as in the case of stable NVs, these turn out to be unattested.

In conclusion, one of the most commonly cited cases of high vowel denasalisation, *i.e.* the French case, fails to provide evidence for the claim that high NVs denasalise earlier than low ones. In addition, since the non-existence of high NVs does not imply denasalisation, languages with nasal but no high nasal vowels, rarely if ever point to the historical denasalisation of high vowels. This means that the nasalisation and denasalisation phenomena in French discussed here cannot be turned into an argument for the HM.

3.1.2 Chinese: nasal vowels via merger and loss of final nasal consonants

The second piece of historical evidence for the HM that I want to look at here is the development of NVs and disappearance of final NCs in many Chinese varieties. The main source for this evidence comes from a series of publications by Chen [88, 89, 90, 91, 92], from Chen & Wang [93] and the references therein. The Chinese groups Chen discusses are, from north to south: North Mandarin, North-West Mandarin, South-West Mandarin, South-East Mandarin, Wú, Hui, Gàn, Xiang, North Mǐn, South Mǐn, Hakka, Yuè (Cantonese) [pp. 17–23]. The main proposal is that the apparent final NVs developed from VN-sequences via, first nasalisation of the vowel by the final nasal and, secondly, deletion of the final NC.

More specifically, Chen claims that NVs "tend to occupy the lower portion of the vowel space" [91, p. 16]. To demonstrate this, let me provide one of Chen's tables [91, p. 25] (this volume, p. 109).

Table 3.1: Frequency of nasal vowels in Chinese dialects according to height and position (Chen [91])

	FRONT			CENTRAL			BACK			$\sum_{\text{FRONTNESS}}$		
	$\sum$	NV	$\frac{NV}{\Sigma}$	$\sum$	NV	$\frac{NV}{\Sigma}$	$\sum$	NV	$\frac{NV}{\Sigma}$	$\sum$	NV	$\frac{NV}{\Sigma}$
HIGH	4672	620	13%				1882	34	2%	6554	654	10%
MID	3066	847	28%	2140	3	0%	3287	167	5%	8493	1017	12%
LOW	202	191	94%	5165	1028	19%	415	166	40%	5782	1385	24%
$\sum_{\text{HEIGHT}}$	7940	1685	21%	7305	1031	14%	5584	367	7%	20829	3056	15%

Table 3.1 is based on 20829 lexical entries (taken from various Chinese dialects) which contain a word-final NC in their corresponding Middle Chinese form. Of these, 3056, *i.e.* 15%, exhibit a final NV in the modern varieties instead of the Middle Chinese VN-sequence. Additionally, table 3.1 illustrates the observation that low NVs are more common than mid NVs, which are more common than high NVs. For example, in this sample 40%, 5% and 2% of all low back, mid back and high back vowels (respectively), contain a NV. Similarly, front NVs are more frequent than central ones, while these in turn are more frequent than back nasal vowels. So while 7% of all back vowels in the sample are nasal, 14% of all central and 21% of all front vowels are nasal. In short:[8]

[8] '$\gg$' stands for 'is more common than'.

(5) *Frequency of nasal vowels (Chen)*

1. LOW ≫ MID ≫ HIGH
2. FRONT ≫ CENTRAL ≫ BACK[9]

To explain the generalisations in (5), Chen proposes the following hypotheses:

(6) *Chen's three hypotheses*

1. Nasalisation
   (a) "Nasalization tends to spread from low to high vowels" [91, p. 16].
   (b) "... nasalization is more likely to be triggered by an anterior nasal (-m, n) rather than a posterior (-ŋ) ending." [*ibid.*]
2. Denasalisation
   (a) "Denasalization progresses in the opposite direction" [*ibid.*].
   (b) NVs have "equal lifespan" [*ibid.*].
3. NV lowering
   (a) "NV's tend to fall" [*ibid.*].
   (b) Hypothesis 3a "is ambiguous, as NV's shift in unpredictable [*sic*] directions" [*ibid.*].

Corollaries of these hypotheses are that low vowels are the most likely to become nasalised (*cf.* hypothesis 1a) and also to resist denasalisation (*cf.* hypothesis 2a). However, both hypotheses 2 and 3 are problematic. Hypothesis 2a states that denasalisation progresses contrary to the direction of nasalisation; so Chen claims that denasalisation starts with high vowels, while low vowels get denasalised last. Hypothesis 2b proposes that NVs have equal lifespan. This predicts that those NVs which were nasalised first are the first to be denasalised and similarly, those NVs which got nasalised last are also the last to be denasalised. Since according to hypothesis 1a low vowels nasalise first, they should then also be the first to denasalise,

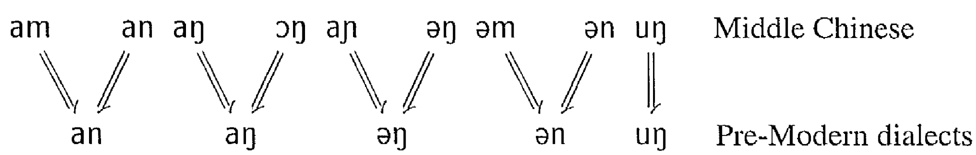
[9]I will neglect here that, contrary to this generalisation, low and mid central vowels in table 3.1 are less common than their corresponding back vowels.

which is precisely the opposite of what hypothesis 2a predicts. In other words, 2a and 2b are mutually exclusive.

The logical problem with hypothesis 3 is that while 3a predicts that NVs "tend to populate the lower corner of the vowel triangle" [p. 16], 3b claims that movement by NVs with respect to vocalic height is unpredictable and can "be regarded as part of the general rising tendency of tense or long vowels (... by compensatory lengthening)" [*ibid.*]. Clearly, 3b ensures that 3a can never be wrong, which makes this statement regarding NV lowering non-empirical. As in the case of hypothesis 2, Chen's combination of assumptions self-destructs.

Having dealt with hypotheses 2 and 3, let us now look at 1. Chen's arguments for the latter assumption can be summed up in the following way: Firstly, based on data as in table 3.1 Chen shows that there is a tendency for NVs to be low. His explanation (hypothesis 1a) is that, diachronically, nasality spreads from low to high vowels. Secondly, Chen assumes five different word-final VN-sequences for Pre-Modern dialects, derived from nine such Middle Chinese sequences (*cf.* figure 3.1):

Figure 3.1: The development of Pre-Modern Chinese VN-sequences from Middle Chinese according to Chen [91]



In Chen's view, there are arguments for saying that not only vowel height influences the probability of nasalisation but also the quality of the final consonant (before its deletion). Chen proposes that in many Chinese varieties with the three-way contrast m n ŋ for final NCs, m and n tend to merge in n; the resulting system with the two-way contrast n ŋ tends to merge in ŋ (figure 3.2).[10]

Figure 3.2 shows the distribution of VN- and Ṽ-sequences across a number of Modern Chinese varieties. It also illustrates in what way Chen, based on this typological pattern, reconstructs the diachronical process Pre-Modern VN-clusters underwent. Note that in Chen's view a dialect at Time 1 may reach the system at Time 3 *either* via Time 2a. *or* Time 2b. So before all final NCs merge into ŋ, either n and ŋ merge into ŋ (resulting in a system with Vm Vŋ at Time 2a.) or m and n merge into n (resulting in Vn Vŋ at Time 2b.). In the emerging system which only allows ŋ as

[10] *Cf.* [88] and [89, p. 41].

Figure 3.2: Merger and deletion of final NC in Chinese (Chen)

Time					
1	m	n	ŋ	Guangzhou, Xiamen, Meixian
2a.	m		ŋ	Chaozhou
2b.		n	ŋ	Peking, Xian, Jinan, Suzhou, Changsha
3			ŋ	Wenzhou, Fuzhou, Taiyuan, Shanghai
4			V^N	Changshou
5			Ṽ	Jinggu, Shuangjiang
6			V	Fengyi, Lijiang

word-final NC, ŋ nasalises the preceding vowel and, in some varieties, disappears. A few dialects (Fengyi, Lijiang) even lose the nasality on the now final vowel.

As Entenman [153, pp. 150f.] points out, Chen is not very clear about interaction between NC merger, on the one hand, and NC deletion and nasalisation, on the other. So according to Chen [88, pp. 116–127], Xian, Jinan, Suzhou and Changsha are not really at Time 2b. but actually have Ṽ instead of Vn, *i.e.* the system Ṽ Vŋ. Chen's problem is that a language at Time 2b. might develop into either one of the systems Vŋ or Ṽ Vŋ (figure 3.3, p. 112); so Vn Vŋ might either develop into Vŋ directly or, as a result of nasalisation of V before n (but not before ŋ) and subsequent deletion of final n, via Ṽ Vŋ (1. V $\Rightarrow$ Ṽ /_ n, 2. n $\Rightarrow \emptyset$ /_ ♯).

Figure 3.3: Two options of language change for the Chinese system Vn Vŋ

Time		
2b.	Vn	Vŋ
2b′	Ṽ	Vŋ
3		Vŋ

So Chen is not able to specify under which circumstances a language would choose to continue the merger of all NCs into ŋ (*i.e.* when the system at Time 2b. changes directly into the one at Time 3) or when it would rather nasalise the vowel preceding final n and, more or less simultaneously, delete this NC (*i.e.* when the system converts from Time 2b. to Time 3 via Time 2b′). In other words, it remains unclear when vowel quality (*cf.* hypothesis 1a) and when consonant quality (hypothesis 1b) is more likely to have an influence on nasalisation or merger. Obviously, hypotheses 1a and 1b apply when they apply (*cf.* strategy 2, section 1.3.2, pp. 25ff.); therefore, like hypotheses 2 and 3, hypothesis 1 fails.

Let me add here that the value of Chen's findings becomes even more questionable when one takes into account the possibility of borrowing across the various dialects Chen uses in his sample. Entenman [153, pp. 157ff.] reports that in Xiamen, a South Mǐn dialect, Chen [88] finds a higher percentage of low NVs than of high NVs which Chen takes as evidence for saying that this pattern was conditioned by vowel height. As Entenman [*ibid.*] points out, Haudricourt [208][11] shows that Xiamen developed Middle Chinese min into lowered mẽ and additionally borrowed this root as non-lowered bin. This reduces the value of Chen's version of the HM even more: Since it is not clear how much borrowing of forms ending in a $V_{HIGH}N$-sequence actually occurred (without nasalisation of the vowel), such cases might well have been common enough to lower the relative frequency of high NVs to such an extent that Chen would have been moved to claim that there is a correlation between vowel lowness and vowel nasality.

Finally, further evidence against the unidirectional merger into ŋ as proposed by Chen comes from Zee [514] and Hess [217]. Zee found that, firstly, there are *two* main directionalities with respect to the historical merger of final NCs in Chinese dialects: -m $\Rightarrow$ -n, and -ŋ $\Rightarrow$ -n. He argues secondly that there is a strong tendency for NVs to develop from final n (-Vn $\Rightarrow$ -Ṽ) and thirdly that the merger -n $\Rightarrow$ -ŋ is, contrary to Chen's statements, a rare phenomenon. Zee therefore explicitly rejects Chen's analysis:

> "Thus, we refute the theory of unidirectionality of the merging of the syllable nasal endings in Chinese as proposed by Chen (1972 [88], 1973 [89])" [514, p. 291].

Hess [*ibid.*], on the other hand, found that, opposed to Chen's views,

1. "nasalization … affect[s] low and mid vowels in one unitary [*sic*] process, rather than in stages as a function of vowel height and nasal place of articulation" (Hess [217, p. 102])

[11] Entenman [153, pp. 158, 328] refers to this publication from 1970 as "Haudricourt (1973)".

2. "raising of vowel height in Wenling and other Wu dialects is not a result of nasalization, but a function of general [*sic*] raising processes" [*ibid.*]

Apparently, the scientific status of Chen's evidence and conclusions is questionable.[12]

To conclude, it has become apparent now that Chen cannot provide conclusive evidence for his claims that in a number of Chinese dialects nasalisation tends to spread from low to high vowels and that historical final NCs merge into ŋ before nasalising the preceding vowel.

3.1.3 The Teke language group: nasalisation before Proto-Bantu °a, °e, °o

The Teke language group is a member of the Bantu-family. While NVs are common in Niger-Congo languages,[13] they are quite rare amongst Bantu languages, a subgroup of the Niger-Congo branch. For the purposes of my discussion of the HM, it is the presence of NVs in the vocalic systems of a number of Teke languages which make these stand out. To be more precise, Hombert [222, 223] claims that the development of NVs in certain Teke languages is phonetically conditioned or, more specifically, dependent on vowel height. According to Hombert, low vowels were nasalised earlier in the Teke group than non-low ones. The point of this section is, of course, to show that there is no conclusive evidence for the claim that lowness, a phonetically defined notion, plays any relevant part in the explanation of this phenomenon.

Hombert [222] starts out by referring to Chen's [92] findings that the more front a vowel or consonant is the earlier it is affected by historical nasalisation. Hombert also points to the claim that "Nasalization affects low vowels first, mid vowels second and high vowels last" [p. 360].[14] In sections 3.1.1 and 3.1.2, I have shown that there is no conclusive evidence for any of these proposals. Apparently, Hombert is not aware

[12]Needless to say that Hess motivates her disapproval of Chen's analysis phonetically. For example, Hess assumes that there is "phonetic evidence which indicates that both mid and low vowels are more likely to be articulated with a lowered velum". Not surprisingly, her phonetic 'evidence' is in no way conclusively phonetically motivated. Like all phonetician-phonologists, Hess simply equates statistically signifant frequency of correlation between phonologically (not phonetically) established nasalisation and some other phonetically defined notion with a causal relationship from phonetics to phonology.

[13]*Cf.* Hombert [222, p. 359] referring to Bolé-Richard [53], Stewart [460, 461] and Williamson [498]. Since Williamson [498] is a reference to a presentation at Yale University, let me refer to two of Williamson's published works on Ịjọ: [499] (two years after [498]) and [497].

[14]Implicitly, Hombert [222, pp. 360f.] uses Ruhlen [421] as reference for this statement but does not consider it to be part of Chen's proposals [92] which in fact it is (*cf.* section 3.1.2, hypothesis 1a, p. 110).

of this. However, since "in order to arrive at more general diachronic statements [*i.e.* more general than only for French and Chinese], additional examples of attested nasalization of vowels are needed" [p. 361]. To find such examples, he investigates the Teke language group. Let me sum up his results:

Hombert compares five Teke languages: Ibali, Ndzindziu, Ngungwel, Fumu and Kṳkṳa.[15] Table 3.2 (p. 116) gives a representative overview over the kind of cross-linguistic correspondences Hombert discusses [pp. 362–370, 378f.].[16] Table 3.3 (p. 153) provides examples from the languages referred to in table 3.2.[17]

Tables 3.2 and 3.3 show that Fumu and Ngungwel are at the extremes of a typological hierarchy: in Fumu there is no nasalisation while, compared to the other varieties discussed here, Ngungwel nasalisation is the least restricted case; nasalisation in Ngungwel occurs with any nasalised type of °C_2 independently of the quality of °V_1.[18] For Ndzindziu and Ngungwel, Hombert can show (based on Guthrie [193]) that there is no nasalisation when °C_2 is °ŋg.

Neglecting gaps, Ndzindziu nasalisation exhibits no restriction on Proto-Bantu °V_1 and occurs with °m or °mb as °C_2, which makes Ndzindziu the second most extreme case of nasalisation portrayed here. Since the restrictions on nasalisation in

[15]His data for Ibali, Ndzindziu and Ngungwel come from Guthrie [191, 193] and "Guthrie, M. (1960)" [p. 375], and for Ibali and Ndzindziu from Guthrie and his own research. The Fumu facts are based on Calloc'h [76] and Mboukou [319] and the Kṳkṳa data on Paulian ([364] and (then) unpublished material by Paulian [363]). Note that 'Guthrie (1960)' in Hombert's bibliography is an article entitled "Teke radical structure and common Bantu", published in the *Journal of African Languages*, volume 1, pp. 1–15; however, this journal's first volume appeared in 1962, and contains a different article by the same author [192].

[16]In spite of Hombert's diligence in providing numerous tables, he did not design a table similar to table 3.2 which would have neatly summed up the evidence for his proposals. '+' stands for presence, '−' for the absence of nasalisation in the modern version of a Proto-Bantu sequence (as defined in the corresponding *Proto-Bantu* cell.)

[17]Gaps in tables 3.2 and 3.3 correspond to gaps in Hombert's data. j̩ and ṳ symbolise so-called 'super-closed' vowels [222, p. 374, footnote 6] which are (supposedly) part of Proto-Bantu's seven vowel system: °j̩ °ṳ °i °u °e °o °a [*ibid.*]. Hombert does not provide a reference for this reconstruction. Regarding Proto-Bantu, I will neglect here that firstly, the reconstruction of any language is dependent on the theory of language change applied and that, secondly, this results in circularity inherent in universals based on historical data. '$\overset{o}{\sim}$' stands for 'optional variation'. For those rows in table 3.3 which do not exhibit specific lexical entries in the Proto-Bantu column, *e.g.* °-umba (6d.), or which display optional nasalised forms in the Kṳkṳa column, *e.g.* $\overset{o}{\sim}$eẽ (6e.), Hombert does not indicate tones. Also, to initiate a more consistent usage of symbols throughout my work, I have changed Hombert's y, ts, tš, j to j, ʦ, ʧ, ɟ, respectively.

[18]'°C_2' and '°V_1' in °$(C_1)V_1(C_2)V_2$-stems. Note that °C_2 may refer to prenasalised stops like °mb. Hombert only spuriously refers to the velar °ŋ or °ŋg. About °ŋ, he writes that "Plain velar nasals are rare in Proto-Bantu and have not been considered here" [p. 374, footnote 13]. Due to the non-availability of an appropriate informant, Hombert cannot provide data regarding nasalisation triggered by °ŋg-clusters from Ngungwel. The informant situation also appears to be the reason for the general lack of specific Ngungwel examples in table 3.3.

Table 3.2: Nasalisation in Teke languages (summary)

	Proto-Bantu	*Fumu*	*Kṳkṳa*	*Ibali*	*Ndzindziu*	*Ngungwel*
1	°V_1ŋV_2					
2	°V_1ŋgV_2			–	–	
3	°V_1nV_2			–	–	+
4	°V_1ndV_2			–	–	+
5	°V_1mV_2	–	–	–	+	+
6a.	°jmbV_2	–	–	–	+	
b.	°ụmbV_2	–	–	–	+	
c.	°imbV_2					+
d.	°umbV_2	–	–	–	+	+
e.	°embV_2	–	$\overset{\circ}{\sim}$	+	+	+
f.	°ombV_2	–	$\overset{\circ}{\sim}$	+	+	+
g.	°ambV_2	–	$\overset{\circ}{\sim}$	+	+	+

Fumu, Ndzindziu and Ngungwel are independent of the quality of °V_1, these three languages do not provide any evidence for or against the HM. In Ibali, on the other hand, nasalisation only occurs when °C_2 is °mb and when °V_1 is a low vowel, *i.e.* °e, °o or °a. Kṳkṳa behaves almost identically to Ibali, the only difference being that while nasalisation is obligatory in Ibali, it is optional in Kṳkṳa. So Ibali and Kṳkṳa show in Hombert's view that

> "parallel to ... universal tendencies ['established' by a questionable analysis of French and Chinese data] ... nasalization occurs first when the vowel *preceding* [Hombert's emphasis] the nasal consonant is low and affects high vowels at a later stage" [p. 371].

This typological pattern can be summed up as in figure 3.4 (p. 117). The directionality of the typological hierarchy illustrated there is upwards. This is to say that the set of conditions under which nasalisation occurs in any one of the five Teke languages in question includes the set of conditions under which nasalisation occurs in all languages above that language. In other words, at the top we find the most restricted case: Fumu, with no nasalisation. At the bottom, there is Ngungwel, the freest case, with nasalisation (almost) independent of the quality of °C_2 or °V_1.

Note however that table 3.2 and figure 3.4 do not take into account whether or not Proto-Bantu '°C_2' remains unchanged in the modern dialects listed. Furthermore, the Kṳkṳa data Hombert provides in the relevant tables [pp. 365, 378f.] do not show

Figure 3.4: Typological arrangement of Teke languages according to nasalisation patterns

↑ implied	Fumu	*(no nasalisation)*
	Kṳkṳa	°V_1 = °e, °a, °o; °C_2 = °mb *(optional)*
	Ibali	°V_1 = °e, °a, °o; °C_2 = °mb *(obligatory)*
	Ndzindziu	°C_2 = °mb, m *(obligatory)*
implies	Ngungwel	°C_2 = °mb, m, nd, n *(obligatory)*

any optional nasalisation of low vowels in V_1-position. He only states that Kṳkṳa VVmV-sequences, having developed from Proto-Bantu °V_1mbV_2-clusters, can according to Paulian [p.c.] also be pronounced VṼ; he does not specify whether this is only possible for low vowels. Optional nasalisation in Kṳkṳa should therefore operate irrespective of vowel quality. However, the three examples for this alternation which he gives (-eeme $\overset{\circ}{\sim}$ eẽ, -aama $\overset{\circ}{\sim}$ aã, -oomo $\overset{\circ}{\sim}$ oõ) do all contain low vowels [p. 364]. Elsewhere [p. 371] he claims that Kṳkṳa nasalisation occurs in "exactly [those] contexts where we find *obligatory* [Hombert's emphasis] nasalization in Ibali", *i.e.* contexts which "correspond to Proto-Bantu forms in which *V_1 was non-high [*sic*] and *C_2 was a prenasalised *labial* [Hombert's emphasis] stop" [*ibid.*]., *i.e.* in the context of $V_{\text{-HIGH}}$mbV_2. The most precise statement regarding this is that, according to what Paulian told Hombert, Kṳkṳa has optional nasalised forms of the type described above, and this nasalisation is probably restricted to those contexts in which Proto-Bantu is assumed to exhibit low vowels. So if, as in table 3.2 and figure 3.4 (and probably according to what Hombert means), nasalisation occurs only with mb as °C_2 and $V_{\text{-HIGH}}$ as °V_1, Kṳkṳa nasalisation looks like an optional version of the corresponding Ibali phenomenon.

In light of this evidence, it appears justified to say that of all the Teke languages Hombert discusses only Ibali and possibly Kṳkṳa display nasalisation restricted to a subset of vowels. There is, however, no evidence at all which would suggest that it is the lowness of °a, °e, °o which selects them as one natural class. On the contrary, if it were the lowness, a phonetically motivated notion, it would remain suspiciously open why in around half of all the languages with NVs, the set of NVs is a copy of the set of *all* OVs present in that system, not a copy only of the set of low OVs.[19] Further-

[19] *Cf.* Hombert [222, pp. 359f.] pointing to Crothers [121] as source for this observation.

more, if pre-modern Ibali and recently some Kụkụa speakers started to nasalise low but not high OVs (resulting in the present-day systems), why did this phonetically driven process not apply in the same manner to pre-modern Ndzindziu and Ngungwel speakers? The phonetically motivated urge to nasalise low vowels first and to spread this phenomenon to other less low vowels must have resulted in one of two possible outcomes. The first option is that the assumed nasalisation of low vowels had set in much earlier for pre-modern Ndzindziu and Ngungwel speakers than for their Ibali and Kụkụa contemporaries and that in the modern varieties nasalisation has therefore proceeded to the high vowels. The second option is that preferential nasalisation of low vowels as proposed must have been ignored by the Ndzindziu and Ngungwel speakers, resulting in the attested nasalisation of low *and* high vowels in these languages. Since Hombert cannot give any reason as to why such differences arise, neither of these options have any explanatory value. In addition, the fact that Fumu speakers could and still can avoid nasalisation so completely must be seen as a mysterious accident by the phonetically driven.

Finally, Hombert fails to come up with any argument for his claim that °a, °e, °o were nasalised earlier in Ndzindziu and Ngungwel than in Ibali and Kụkụa. The presence of one Bantu system without nasalisation, two with nasalisation irrespective of vowel quality and two with nasalisation exclusively of reconstructed °a, °e, °o does not show conclusively that in the quality-independent cases, *i.e.* in Ndzindziu and Ngungwel, °a, °e, °o were nasalised earlier than Proto-Bantu °i̧ °u̧ °i °u. Similarly, there is also no evidence to claim that in "Fumu, nasalization has not yet started" [p. 371]. To make a statement of this kind for Fumu is as nonsensical as it is to say that Turkic-style 'roundness' harmony (U-harmony) [86, 87] or nasalisation of 'low' vowels 'has not yet started' in German or English. Some might argue that the non-occurrence of nasalisation in Fumu cannot be compared to the non-existence of U-harmony in, let us say, German because there is no evidence from languages related to German where U-harmony *has* started. This argument, however, can only be relevant to those who do not mind that their theories predict that in order for a child to acquire Fumu (or German), s/he has to find out first whether or not there is a nasalisation (or U-harmony) process in any varieties related to Fumu (or German). A child acquiring Fumu does not need evidence from Ibali in order to acquire it as much as a child acquiring German does not need evidence from any variety other than the one s/he is acquiring. Consequently, the relationship between a child acquiring Fumu and the non-existence of nasalisation in Fumu is identical to the one between a child acquiring German and the non-occurrence of U-harmony in that variety of German. So I maintain that there is no evidence for Hombert's claim that nasalisation in Fumu has not yet started. This also means that even if in future some or all vowels in Fumu become nasalised under certain conditions, Hombert's claim would still not be a scientific assumption (strengthened by a correct prediction) but merely a lucky guess, an inkling, so to speak.

To sum up, there is no evidence for Hombert's assumption of a phonologically relevant correlation between nasality and lowness nor for his claim that in the pre-modern varieties of a number of Teke languages low vowels were nasalised earlier than high ones. Apparently, the HM remains a myth.

3.1.4 Romagnol dialects: nasalisation of Stage 2 °a

The final piece of historical evidence in favour of the HM which I would like to look at comes from a number of Romagnol dialects (Northern Italy). The most recent source for nasalisation phenomena in this area is Hajek [196, *e.g.* p. 51] who discusses the universals of historical change in nasalisation.[20] This section on Romagnol dialects has two parts: First I will discuss a case of preferential nasalisation of (reconstructed) °a and argue that this case too does not provide evidence for the HM (section 3.1.4.1). Subsequently, in the second part, I will briefly point to an alternative explanation of the Romagnol data (as pointed out by Hajek [196]); this explanation is based on a proposed correlation between nasality and length. In order to evaluate this analysis I will suggest that an assumed nasality-length correlation, if phonologically relevant at all, can only be so if it is established independently of any version of the PH, *i.e.* independently of phonetically defined length (section 3.1.4.2).

3.1.4.1 The Height Myth and the Romagnol dialects

I will start by providing a brief introduction to a few relevant concepts relating to the reconstruction of the Romance languages [196, pp. 40ff.].

Hajek assumes two main stages to get from Latin to the Northern-Italian and Western Romance languages: 'Stage 1' and 'Stage 2'. The most important changes characterising Stage 1 are [p. 41]:[21]

[20]Hajek's sources for Romagnol dialects are Schürr [437, 438, 439, 440, 441], Bottiglioni [55], Pelliciardi [365], Baldassari [24], Bellosi & Quondamatteo [39] and Delmonte [134].

[21]Hajek [*ibid.*] mainly refers to Rohlfs [415], Saltarelli [426], Swiggers [469], Väänänen [481] and Vincent [484].

(7) *Stage 1 changes: From Latin to Stage 1*

1. Reduction of the Latin vowel system.

iː	i		eː	e	aː		a	o	oː		u	uː	Latin
⇓	⇘		⇙	⇓	⇘		⇙	⇓	⇘		⇙	⇓	
°i		°e		°ɛ		°a		°ɔ		°o		°u	Stage 1

2. Deletion of (most) final NCs, *e.g.*

kanem	paːnem	balneum	Latin
'dog'	'bread'	'bath'	
⇓	⇓	⇓	
°kane	°pane	°banju	Stage 1

3. Palatalisation.

centu	gente	Latin
'hundred'	'people'	
⇓	⇓	
ʧento	ʤente	Italian

Since vowel system reduction, final NC-loss and palatalisation cannot only be found in isolated cases but are characteristic for Romance languages in general, data as in point (7) show in Hajek's view that these developments must have taken place quite early in the development of the Romance languages.

At a later date, Stage 2 changes are assumed by Hajek to have occurred in Northern Italian, Rhaeto-Romantsch and many Western Romance dialects (like Bolognese and Tavetschan in (8)), thus distinguishing them from Central and Southern Italian varieties (like Standard Italian)[22] [p. 41]. They are [p. 45]:

[22]Like Standard Italian except point 4 in (8), where Italian in line with Bolognese and Tavetschan opts for the fusion of Latin °nj to Stage 2 °ɲ. Varieties in which °nj remained unfused are, for example, Sardinian and Corsican. In agreement with Contini [116] and Rohlfs [415] Hajek finds evidence for unfused °nj in these varieties; the modern Sardinian and Corsican forms of Stage 1 °vinja (from Latin vinea 'vineyard') are: Sardinian binʤa and Corsican binɟa [p. 45].

(8) *Stage 2 changes: From Stage 1 to Stage 2*

1. Early lenition of Latin p t k b d g s f in intervocalic context.

Latin	*Italian*	*Bolognese*	*Tavetschan*	
rota	rwɔːta	roːda	roːdə	'wheel'
kauda	koːda	ko	kuːə	'tail'

2. Degemination.

Latin	*Italian*	*Bolognese*	*Tavetschan*	
vakka	vakka	vaːka	vakə	'cow'

3. Deletion of final unstressed vowels other than a.

Latin	*Italian*	*Bolognese*	*Tavetschan*	
lakte	latte	laːt	lac	'milk'
barba	barba	bɛːrba	barbə	'beard'

4. Fusion of Latin °nj to °ɲ.

Latin	*Italian*	*Bolognese*	*Tavetschan*	
°banjat	baɲɲa	baːɲa	boɲa	'bathes'

The evidence in favour of the HM stems from assumptions about this stage of the development of the Romagnol varieties. The most common context for a Stage 2 vowel °V to nasalise is within a °Vːn♯-sequence, though in this environment, nasalisation in Romagnol dialects takes place independently of vocalic height. Hajek's [p. 121] examples are:

(9)

°paːn	°viːn	°karboːn	Stage 2
'bread'	'wine'	'coal'	
⇓	⇓	⇓	
pɑ̃ː	vĩː	karbũː	Milanese

However, in Imolese, Ravennate and other Romagnol varieties, Stage 2 °a, but no other vowel, becomes nasalised when it either fills the °V_1-position in a °V_1n(V)- or °V_1NC_{STOP}-V-sequence (10a.)[23] or when preceding Stage 2 °ɲ (10b.):

[23]Hajek [p. 121] refers to Schürr [439] as source for this.

(10) *Stage 2 developments in Imolese*

a. Nasalisation of Stage 2 °an(V) and °aNC$_{STOP}$-V

	Stage 1			*Imolese*
	°kanna	'cane'	⇒	kɛ̃ːna
	°gamba	'leg'	⇒	gɛ̃ːmba
but:	°membru	'member'	⇒	membar
	°pinna	'feather'	⇒	pena

b. Nasalisation of Stage 2 °aɲ

	Stage 1			*Imolese*
	°banju	'bath'	⇒	bɛ̃ːɲ
but:	°venio	'I come'	⇒	veɲ

Obviously, a (as opposed to other vowels) was favoured by nasalisation. However, the mere fact that, in some phonological environments, reconstructed °a but no other vowels were nasalised does not suggest in any way that the lowness of °a is responsible for this. If lowness were the relevant factor it would remain unclear why the lowness of the pre-modern Ibali and Kṳkṳa versions of the Proto-Bantu mid-vowels *was* low enough to trigger nasalisation of these vowels (*cf.* section 3.1.3) while in the Romagnol dialects the lowness of the Stage 2 developments of Stage 1 mid-vowels, *i.e.* °e °ɛ °o °ɔ (*cf.* point 1 in (7)), was not low enough to initiate the development of nasalisation (in a restricted context). Note also that such an assumption would leave unaddressed the question in what way Stage 2 °ɲ provided the degree of lowness, backness (or any other phonetically defined concept) necessary to trigger nasalisation of the preceding vowel.

Furthermore, the attested nasality-height correlation might be due to other factors. Independently of what these other factors actually are, it can be said that as long as there is no *conclusive* evidence in favour of any nasality-height correlation or affinity, the HM is as likely to hold true as it is not to hold true and is thus irrelevant. More specifically, even if the assumption of the relevance of the concept 'height' were not questionable in itself, a supporter of the HM would still have to deal with the problem that as long as there is no conclusive evidence for the claim that it is the lowness in °a which is responsible for the ease of nasalisation observed, there is no reason why the phonetically motivated story should be the relevant one. Obviously, it is as explanatory to assume that phonetics is relevant as it is not to assume this, which means that the assumption of the relevance of the concept 'lowness' has no explanatory value for the Romagnol data at all.

3.1.4.2 Nasality-length correlation as explanation of the Romagnol data

As pointed out above, it is possible to explain the Romagnol data presented in section 3.1.4.1 without crucial reference to any nasality-height affinity, and it is the existence of explanations alternative to the HM which make the HM inconclusive. In this section, I will present one such alternative explanation, which has been put forward by Hajek. He points out that

> "The apparent vowel-height related pattern of nasalization may simply be an indication of a recent change in the productivity of nasalization: nasalization, governed by the Vowel Length Parameter (VLP), and productive when Stage 2 /a/ was lengthened, may have ceased to be productive by the time /ɛ/ was lengthened. If this is the case, then operation of the VLP, independent [*sic*] of the VHP [Vowel Height Parameter], would be sufficient to account for restricted low vowel nasalization where reported in Romagnol." [p. 122]

To understand this, let me provide Hajek's definitions of the VHP and the VLP. Hajek [p. 116] formulates the VHP in line with Chen's claims about universals in nasalisation (*cf.* point 1 in (5) and point 1a in (6)) like this:

(11) *Vowel Height Parameter*

LOW MID HIGH
$\longrightarrow$

Via the VHP, Hajek expresses the assumption (with which he does not agree) that

> "the development of some or all parts of the distinctive nasalization process occurs preferentially in the context of low vowels before spreading gradually to mid and then finally to high vowels in pre-nasal position" [p. 116].

Based on Hombert's observation that in Teke languages the nasalisation of VN-sequences presupposes the nasalisation of VːN-clusters and confirmation of this implication in other languages,[24] Hajek formulates the VLP in the following manner

[24]This includes German dialects (Schirmunki [435], Thinnes [473]), Veracruz Nahuatl (Karttunen [246]), Ceylon Gypsy Telugu (Karunatilake [247]), certain Irish Gaelic varieties (Breatnach [64]), Sardinian and Corsican dialects (Contini [116], Dalbera-Stefanaggi [124]), the Gallo-Romance dialect of Damprichard (Grammont [182, 183, 184]) and a number of Indo-Aryan languages (Bloch [51])

[p. 88]:

(12) *Vowel Length Parameter*

VːN VN

⟶

The VLP expresses that nasalisation of a vowel which precedes a NC develops preferentially with long vowels and tends to spread to short vowels in the same context. In Hajek's view, there is little evidence for the VHP but a substantial amount of evidence for the VLP.

Let me now add a final piece of information. In a number of Northern Italian dialects, Stage 2 °a, and later higher vowels, *e.g.* °ɛ, were lengthened [p. 47] when stressed and (simultaneously) in open syllables [p. 38]:

(13)

Latin	*Stage 1*	*Stage 2*	*Bolognese*	
karu	°kaːru	°kaːr	kɛːr	'dear'
karru	°karru	°kar	kaːr	'cart'
pasta	°pasta	°pasta	paːsta	'pasta'

So Hajek's alternative explanation of the attested nasalisation of Stage 2 °a is as follows:

(14)

	a. ˈV ⇒ ˈVː /_ $	b. Vː ⇒ Ṽː /_ n
T1	Stage 2 °a ⇒ °aː	Stage 2 °aː ⇒ °ãː
⇓	⇓	⇓
T2	Stage 2 °ɛ ⇒ ɛː	ɛː, *ɛ̃ː

Nasalisation, as conditioned by the VLP and thus developing ãː from °a at T1 (14b.), might have stopped before °ɛ was lengthened to ɛː at T2 (14a.). ɛː would have 'missed' nasalisation (14b.).

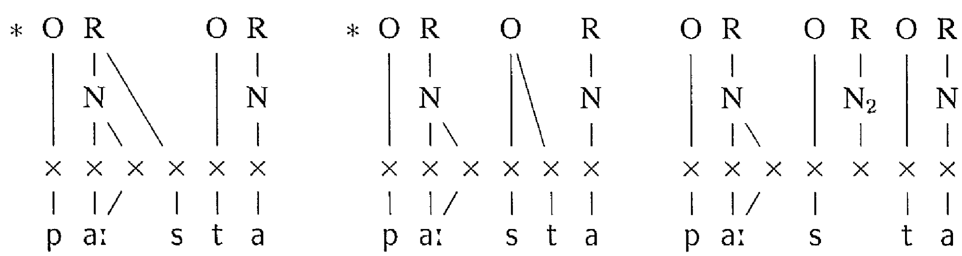
Due to the fact that I agree with Kaye's [256] analysis of sC-clusters as heterosyllabic sequences—usually 'coda'-onset sequences—I have to discuss the observable lengthening of Stage 2 °a to Modern Bolognese aː in paːsta (13). For

[196, pp. 87f.].

argument's sake, let us assume that the phonetic length of aː in Bolognese paːsta is phonologically relevant here. Since the stressed °'a in Stage 2 °p'asta is lengthened when preceding °st (Bolognese paːsta), it follows that stressed °'a in the pre-modern Bolognese interpretation of Stage 2 °p'asta cannot have been linked to the nuclear head of a branching rhyme. In other words, pre-modern Bolognese °'aː—linked to only one nucleus—could not have governed a postnuclear rhymal position ('coda') dominating °s, a structure suggested by Kaye for (most) sC-sequences (*cf.* (2a.) in section 1.5.1, p. 43). The reason for this is that stressed long (pre-modern Bolognese) °'aː would otherwise have to have been dominated by a so-called super-heavy rhyme, a structure that is ill-formed in Kaye's approach (15a.). In such a super-heavy rhyme °'a could have been linked to a branching nucleus which would have governed a postnuclear rhymal position. Since there is good evidence to exclude this option (*cf.* KLV [267]), it can be said that independently of the assumed constituent structure for the Stage 2 version of Modern Bolognese paːsta, pre-modern Bolognese speakers must have assigned it a structure which put stressed Stage 2 °'a in an 'open syllable', or, in phonologically relevant terms, in a structure where it was linked to a non-branching rhyme, *i.e.* one without a 'coda'. Ignoring other evidence (and still assuming the phonological relevance of this manifestation of phonetic length), this leaves only two possible constituent structures for the pre-modern Bolognese interpretation of Stage 2 °pasta: Firstly, the structure in (15b.) with the sC-cluster linked to one branching onset or, secondly, the one in (15c.) with an empty nucleus (N_2) intervening between s and t.

(15) *Alternatives to Kaye's analysis of* sC*-clusters*

a. *Super-heavy rhyme* b. *Tautosyllabic* st c. *Empty nucleus*



Since, as Kaye [256] has shown, sC-sequences are never tautosyllabic, the structure in (15b.)) is ill-formed. So sC-clusters are always heterosyllabic, *i.e.* they are either linked to a 'coda'-onset sequence (where the preceding nucleus governing that 'coda' may not branch) or to separate onsets with an intervening nucleus (where the nucleus preceding s may or may not branch). Due to the observable lengthening of stressed Stage 2 °'a in Modern Bolognese, it follows regarding post-Stage 2 °p'asta that pre-modern Bolognese speakers must have analysed the sC-sequence as illustrated in (15c.), *i.e.* as °p'as∅ta.[25]

Note that it does not matter whether °s and °t in post-Stage 2 °p'asta were linked to a 'coda'-onset sequence (as in Kaye's proposal, with each phonological expression dominated by different skeletal points) or whether they were already associated to a structure as in (15c.) (with an intervening empty nucleus). For argument's sake, let us assume that post-Stage 2 speakers had not lengthened stressed °'a yet and the consonantal cluster °st was linked to a 'coda'-onset sequence. This situation would not imply that the °st-cluster would also have to have been linked to a 'coda'-onset sequence in pre-modern Bolognese since speakers of pre-modern Bolognese might simply have reinterpreted post-Stage 2 °st. Reinterpretation of this kind cannot occur as a phonological process[26] but happens exclusively as the result of language-in-contact situations. For example, °'a in post-Stage 2 Romagnol might have been phonetically (but not phonologically) long when stressed or phonetically longer than °'a in a comparable environment in the language spoken by the future-speakers of pre-modern Bolognese, thus encouraging them to reinterpret phonetic length of a foreign language or dialect, *i.e.* of post-Stage 2 Romagnol, as phonological one. Since, typically, phonetic length might or might not have indicated phonological length, phonological length would in none of the above scenarios have been deducible from phonetic length.

Which of these scenarios can most accurately explain the data involved is of no concern here. What is interesting though is that an explanation alternative to the assumption of a nasality-height correlation is possible: Hajek's explanation based on (a phonetically motivated version of) the VLP rather than the VHP and my elaboration on its phonological background show that there is no conclusive evidence in

[25] '∅' represents an empty nucleus. Note that according to the Empty Category Principle in Government Phonology empty nuclei like N_2 in (15c.) are subject to the Phonological Empty Category Principle ('ECP') and can thus not simply be used to account for inconvenient data. The ECP states that "a p-licensed (empty) category receives no phonetic interpretation" (Kaye [256]). A version of the ECP only relating to empty nuclei was formulated in Kaye [255]. Drawing on Kaye [256, 258], *i.e.* according to the a version of the ECP revised to 1993, there are four types of p-licensed categories: 1. a domain-final empty nucleus p-licensed by parameter, 2. a properly governed category (Charette [82, 83]), 3. a nucleus within an inter-onset domain (Kaye [259]) and 4. a magically licensed nucleus (Kaye [256]). For further discussion, *cf.* Charette [84] and Gussmann & Kaye [190]. In accordance with the ECP, N_2 in (15c.) is silent, *i.e.* not phonetically realised.

[26] The principle in GP excluding such synchronic phonological reinterpretation of structure is the *Projection Principle*; it states that "Governing relations are defined at the level of lexical representation and remain constant throughout a phonological derivation", *cf.* KLV [267, p. 221] for the relevant arguments.

the Romagnol data for any such affinity of nasality to height. However, I would like to discuss briefly whether or not Hajek's VLP is relevant to the study of nasality. In my view, the answer to this question is clearly negative. The reason for this is that the concept 'length' is itself phonetically motivated and is thus too flexible to be of any scientific use. So for some phonologists, it may be relevant to establish length physically, *e.g.* via measurements based on spectograms of the phonetic environment of the vowels in question. Such measurements are due to their phonetic nature of course totally meaningless for phonology. As discussed above, the main problem here is that there is no phonetically based theory where assumed phonological length is predictable from phonetic measurements; phonetic length always might or might not indicate phonological length. Whether phonological length is actually deducible from phonetic length must always be established independently of the very phonetics which is assumed to motivate phonology. In other words, there is no phonological theory known to me where it is not entirely arbitrary when phonetic and phonological length match up and when they do not. This is to say that since for supporters of phonetically motivated phonological length there is no independent way of establishing under which circumstances phonological length is to be defined based on phonetic measurements, it would remain a matter of 'empirical investigation'. This would unfortunately be a case where the matter to be investigated empirically (*i.e.* the phonological relevance of phonetically defined length) would be assumed regardless of whether that (future) investigation is actually untertaken by anyone and regardless of its findings. In other words, it would be assumed that phonetic length is an important factor in defining phonological length independently of whether or not and how often this assumption holds true.

Due to the unfalsifiable status of the PH, phonological length can, as discussed, never be deduced from phonetic length; this includes examples where (irrelevant) phonetic length cannot be measured, as it is usually the case with historical data. The following example will illustrate this point: Hajek, a supporter of the PH,[27] refers to Schirmunski [435] who shows that the group of German varieties where nasalisation and subsequent NC-deletion occurs after historically *short* stressed vowels is a small subgroup of the class of varieties in which nasalisation and NC-deletion is found

[27] *Cf.* Hajek's remark that 'Recent research ... suggests that ... the degree of phonetic concreteness in phonology is far greater than normally assumed, even at deeper levels of grammar" [196, pp. 4f.]. The flexible nature of such phonetic concreteness in mainstream phonology (as explained in chapters 1 and 2) points to a rather abstract notion of concreteness. Alternatively, Hajek's notion of phonetic concreteness might also be based on the non-existence of a well-established phonetic framework (section 1.4.3) as pointed out by Ladefoged [283].

following *long* stressed vowels [196, p. 87]. That is to say that in these dialects nasalisation and NC-loss after short stressed vowels implies nasalisation and NC-loss after long ones, but not *vice versa*. Due to the self-evident difficulties involved in establishing accurate phonetic measurements for historical data, for Hajek those vowels which appear to have been phonologically long are assumed to have been phonetically long, and, based on the PH, it is the assumed phonetic length which motivates the phonological one. Another example Hajek uses to argue for the VLP is Thinnes [473] who proposes that in the Rhein-Frankonian dialects of Wackernheim there are no underlying short NVs and that NC-deletion occurs only following long but not short historically oral vowels.

Assuming the PH, it should theoretically follow clearly which of the vowels concerned are phonologically long. NC-deletion following vowels established as short via phonetics (or assumed to be phonetically short) should imply NC-deletion following vowels established as long via phonetics (or assumed to be phonetically long), but not *vice versa*. Typically, this is not what happens. As in all phonetically motivated approaches, a phonetically based definition always turns out to be flexible enough to 'account for' examples which in a scientific approach would have to be regarded as counterexamples. So if the correlation between nasality and length as observed in German varieties by Schirmunski and Thinnes is actually between phonetically defined nasality and phonetically defined length, one would not only not be able to deduce phonological 'nasality' from phonetic one (*cf.* chapter 1), it would also remain unclear why (supposedly) rare short NVs, "disfavoured" [196, p. 92] by so many languages, are nevertheless in surface if not underlying opposition with long NVs in at least some languages. Hajek points to Lepelley [292, p. 50] who has the underlying contrast /mã/ 'hand' *versus* /mãː/ 'hands' for Norman French. In order to explain the more marked occurrence of such an opposition, Hajek considers it "plausible" [196, p. 94] "that the purportedly short and long nasal vowels are really phonetically long and extralong respectively." [*ibid.*]. Within such an analysis, there would not be any short NVs in Norman French, because short would now be phonetically long (but not extralong!). It is this statement which makes it clear that phonetic length which might be interpreted as non-present (in a phonetically short vowel) might just as well be interpreted as simply present (in form of 'normal' phonetic length), while normal phonetic length, whatever that might be, might also be interpreted as phonetically extralong. The criterion according to which one is supposed to decide whether short means short or normally long and whether long means normally long or extralong is not a phonetic one but an 'empirical' or cognitively phonological one: Hajek motivates a 'phonetic' reinterpretation of what is usually seen as phonetically short or long dependent on whether or not the vowels in question undergo nasalisation, *i.e.* their (phonetically independent) phonological behaviour, and independent of their independently established scientifically measurement-based phonetic qualities. As always, phonology *un*motivated by phonetics is part of what motivates phonetics, and not *vice versa*.

Apparently, Hajek assumes that phonology is motivated by phonetics but motivates what must be phonetically short, long or extralong by historical (*i.e.* non-phonetic) material, which within his approach is classified according to phonetic criteria and would thus have phonological implications. So it is totally unclear which part of his implicit definition of length is phonetically motivated and where precisely he allows himself to ignore phonetics. For him, phonetics provides the kind of scientific jargon vague enough to maintain a point of view which may not explain anything but which will turn out unfalsifiable and thus infallible time and time again.

Note also that most of the languages with NVs do not exhibit underlying *or* surface length distinction for them (*cf.* Ruhlen [419]). Such languages have only one series of NVs, usually classified as 'short'. No matter whether or not the NVs in such languages are somewhat phonetically longer than comparable OVs, the languages with NVs which I have looked at only rarely exhibit phonological evidence which would justify analysing the attested NVs as phonologically 'long'.[28] The questions phonologists who believe in the VLP and thus in the phonological relevance of phonetics avoid to address are, for example: How can languages with one series of ('short') NVs avoid lengthening them (phonetically)? Or is this not even something the VLP predicts? If the phonetic VLP is relevant, what does the correlation between phonetic length and nasalisation stem from? What does the precise definition of this correlation predict? Or how can we be so sure that what we consider to be phonetically short is not phonetically long, or similarly, what we assume to be phonetically long is not phonetically extralong? If the (phonetic) VLP were relevant, one would expect to find evidence that suggests that NVs are lengthening world-wide! Where is that evidence? Or maybe we would not expect that? Who is to tell?

[28]The only phonological phenomena I know of which often result in phonetic lengthening are stress and the linking of a phonological expression to two skeletal points. Opposed to academico-politically correct views, I know of no example where the phonetic concept 'long' plays any role in phonology. In chapter 6 (in particular, (25) on p. 249), I will provide an analysis of NVs in Québec and Montpelier French in which I will claim that the NVs occurring in these languages consists of two skeletal points.

Clearly, either the phonetic explanation of the VLP, like any other explanation, would have to be precise enough to be falsifiable, or it would simply not be an explanation but remain a description at best.[29] Typically, the phonetically based assumption that there is some kind of affinity between phonetically motivated pseudo-phonological concepts like length and nasality is not supported by evidence. This assumption also lacks content to such an extent that worl-wide lengthening of NVs could be regarded as evidence supporting the VLP (by interpreting it as yet another manifestation of the phonetic connection length-nasality) while the apparent non-occurrence of this global lengthening process does not have to have an effect on the formulation of the VLP (and so it does not) and can be completely ignored. This is what I call real flexibility! It appears that due to the irrelevant status of phonetic length in phonology, a V*Length*P is descriptive at best and explains nothing.

To sum up, there is no historical evidence for the phonological relevance of length, height, nasality, correlations between any of these phonetic concepts or, more specifically, Hajek's VHP or VLP. Hajek's VLP, however, requires further analysis. As Hajek and others have shown, co-occurrence of phonetic nasality and phonetic height, on the one hand, and of phonetic nasality and phonetic length, on the other, are historically and synchronically common. The matter to be researched is when this is phonologically motivated and, if so, in what way. Having looked at correlations between nasality and height diachronically, I will deal with this subject from a synchronic perspective in the following. A more detailed discussion of the affinity between nasality and length and of the question whether there is a phonological explanation for this phenomenon is, unfortunately, beyond the scope of this investigation.[30]

3.2 Synchronic evidence

In the following, I will first look at various claims based on synchronic language data that nasalisation in vowels has an influence on their height and and that this can be motivated phonetically (section 3.2.1), and subsequently at the (ir)relevance of many of the data commonly cited to support a synchronic version of the HM. Such data are usually attained from morphology or so-called 'synchronic morphophonemic

[29] For some it might even be a matter of faith.

[30] There is an article on the connection between nasality and vowel duration and height by two phonetics experts, Whalen and Beddor, who find that "the apparent [!] tendency for low vowels to nasalize more readily (historically and synchronically) is not due to the inherent duration difference between low and hight vowels" (Whalen & Beddor [495, p. 482]).

variation' (3.2.2) or from a comparison of modern with historical forms (3.2.3).

3.2.1 Beddor (1982): a perceptual view of synchronic nasality-induced vowel height shifts

In this section I will discuss synchronic evidence which has been used to argue for (some version of) the HM. I will show that the arguments commonly employed to support the claims that there is a phonetically motivated yet phonologically relevant correlation between nasality and vowel height or that vowel height has an influence on vocalic nasalisation or denasalisation are flawed. The main work on this subject is, to my knowledge, Beddor's PhD thesis *Phonological and Phonetic Effects of Nasalization on Vowel Height* from 1982 [34].[31]

Let me go *in medias res*. Beddor investigated a 75-language sample for manifestations of synchronic nasality-height correlations in vowels. Based on this study and cross-linguistic surveys by Bhat [46], Foley [166], Ruhlen [421] and Schourup [436], BKG [36] suggest a number of generalisations which can be summed up as in table 3.4 (p. 153).

Table 3.4 displays "patterns [which] reflect synchronic allophonic and morphophonemic variation between oral and nasal vowel height" [36, pp. 198f.], *e.g.* French [fin] 'nice (f.)' in variation with [fæ̃] 'nice (m.)' [p. 198]. These patterns are dependent on the "interaction of ... vowel height, vowel context, and vowel backness" [p. 199]. So height in NVs is more centralised than in comparable OVs. Context exhibits its influence when nasal segments adjacent to OVs influence potential targets for nasalisation if the targets are vowels of mid height. Context also "distinguishes lowering of mid non-contextual nasal vowels from raising of mid contextual nasal vowels" [*ibid.*]. Vowel backness "primarily affects mid vowels, but a front-back asymmetry holds for all vowels, ... lowering of a back nasal vowel implies lowering of the corresponding front nasal vowel in that language" [*ibid.*].[32]

More specifically, independent of whether nasalisation is distinctive or contextual, high NVs (*e.g.* ĩ ũ in Bengali, Ewe, Gadsup, Inuit and Swahili) are lowered cross-linguistically, low ones (*e.g.* ã in (Plougrescant) Breton, Haida, Nama, Seneca and (Guelavía) Zapotec) are raised. For mid NVs, distinctively nasalised vowels

[31] Further literature can be found in Beddor, Krakow & Goldstein ('BKG') [36] and in the references in [34] and [36]. BKG [36] discuss the role of perceptual constraints in (diachronic) phonological change and argue that "nasalisation affects vowel height only when nasalisation is phonetically inappropriate ... or phonologically inapproriate" [36] in the listener's language.

[32] BKG [36] refer to Beddor [34] and Maddieson [312] as reference for this implication.

must be differentiated from contextually nasalised ones: Distinctively nasalised mid vowels are lowered (ẽ õ in Maithili, Portuguese, Shiriana and Yuchi; ẽ, but not õ, in Hindi, (Jicaltepec) Mixtec and (Kiowa) Apache); contextually nasalised mid vowels are raised if they are back vowels (*e.g.* õ ɔ̃ in (Toba) Batak, Dutch and Nama); if such a contextually nasalised mid vowel is front (ẽ), it is raised if the corresponding back vowel is raised (resulting in raised ẽ and õ, *e.g.* in (Connaught and Scottish) Irish, Basque and (Havyaka) Kannada) but is lowered otherwise (resulting in lowered ẽ but unchanged õ, *e.g.* in (Eastern) Armenian, Campa, Fore and Tewa).[33]

Additionally, Beddor [34] tries to find a phonetic explanation for the perceptual changes in vowel height co-occuring with nasalisation and the phonological raising and lowering of NVs she associates with them and therefore investigates the acoustic characteristics of English, Turkish, Hindi and Igbo "/i/, /e/-/ɛ/, /æ/, /a/, /o/, and /u/ and their nasal counterparts" [34, p. 237]. On the basis of the assumption that phonological NV raising and lowering should be reflected in first formant (F1) frequency of oral and nasal vowels if F1 frequency is responible for phonological NV raising and lowering, Beddor comes to the conclusion that F1 frequency is not responsible for such phonological changes in vocalic height since "In general, these first formant differences were not found" [*ibid.*].

However, Beddor considers it "premature to conclude on the basis of first formant data that acoustic factors cannot account for phonological nasal vowel raising and lowering" [p. 238]. In Beddor's view, perceptual research, *e.g.* by Chistovich, Sheikin & Lublinskaja [95] and Delattre *et al.* [133], points to the relevance of the first region of prominence in vowel spectra in general and not only specifically of F1 frequency values. Since there is a "significant increase in centroid value from [i] to [ĩ] and [e] to [ẽ] and a significant decrease from [æ] to [æ̃] and [a] to [ã] (bilabial context)" [34, p. 238] and from [o] to [õ] [*ibid.*], Beddor suggests that the centroid of the first spectrally-prominent region of the vowels measured is responsible for the attested perceptual changes caused by nasalisation. Note here that Beddor finds *no* significant change from [u] to [ũ] [*ibid.*]. Since Beddor assumes that an increase in centroid value lowers perceived height while a decrease raises it, she expects per-

[33]The references for the relevant language data given in Beddor [34] are (in alphabetical order, according to languages): Apache (Kiowa) [47], Armenian (Eastern) [3], Basque [304], Batak (Toba) [480], Bengali [159, 277], Breton (Plougrescant) [236], Campa [138], Dutch [332], Ewe [11, 43, 453], Fore [340], Gadsup [168], Haida [429], Hindi [156], Inuit [411], Irish (Connaught) [N. Stenson, p.c. with Beddor, *cf.* [34, p. 62]], Irish (Scottish) [136, 355], Kannada (Havyaka) [45, 361], Maithili [511], Mixtec (Jicaltepec) [62], Nama [29, 122], Portuguese [147, 331, 465], Seneca [81], Shiriana [326], Swahili [389], Tewa [221], Yuchi [25, 119], Zapotec (Guelavía) [243].

ceptual lowering from [i] to [ĩ] and [e] to [ẽ] (and thus, in her phonetically motivated story, from /i/ to /ĩ/ and /e/ to /ẽ/) and perceptual raising from [æ] to [æ̃], [a] to [ã] and [o] to [õ] (and thus from /æ a o/ to /æ̃ ã õ/, respectively). Beddor has no expectation about [u] or /u/ "in the absence of significant centroid shifts" [p. 239]. To sum up, Beddor states that

> "... it is proposed that the phonological processes which lower high and mid front nasal vowels and raise mid back and low nasal vowels are motivated by the acoustic-perceptual characteristics of nasal vowels" [pp. 249f.].

Let me now point to the problems I see with Beddor's perceptually based explanation. Firstly, as discussed above, one of the phonetic notions which the patterns reflecting the synchronic variations between oral and nasal height of mid vowels are dependent on is context; for example, while distinctively nasalised mid vowels are lowered, contextually nasalised ones may be raised or lowered, dependent on the backness of the vowels involved and, if they are front, on whether or not the corresponding back vowel(s) in that language is (are) raised (*cf.* table 3.4, p. 153). However, which nasalisation phenomenon is considered to be contextual and which is analysed as distinctive depends on the phonological analysis, *i.e.* for supporters of the PH, on what the PH can and cannot explain. This type of phonological analysis is *independent* of phonetics because phonetic data does not help to decide whether nasalisation is contextual or distinctive. Phonetically defined vowel height shifts, on the other hand, are phonological phenomena in Beddor's account ("phonological nasal vowel raising and lowering" [p. 238]) and can thus be regarded as phonetically motivated phonological events. So this type of phonological analysis is *dependent* on phonetics. Apparently, for Beddor there is a type of phonology motivated by phonetics and one unmotivated by it, and there is no independent criterion according to which it would be possible to predict when phonetics does and when it does not motivate phonology. Therefore, Beddor's phonetically based approach suffers—like all phonetically based approaches I have looked at—from unfalsifiability via strategy 2, *i.e.* flexibility of applicability of the PH (*cf.* section 1.3.2).

The same problem also becomes evident in Beddor's attempt to find out in what way (presumably phonological, *i.e.* phonetically motivated) NV raising and lowering is actually "reflected" [34, p. 237] in the phonetics of the vowels in question, more specifically, in the F1 frequency values of nasal and oral vowels. The 'phonological' raising and lowering referred to here logically presumes the relevance of phonetics (as in 'raising' and 'lowering') for phonology independently of whether there is any phonetic motivation for this assumption. And this is why Beddor has to check whether supposedly phonetically motivated 'phonological' raising and lowering is actually reflected phonetically, *e.g.* in F1 frequencies or centroid values of the first spectrally-prominent region of nasal and oral vowels. Unfortunately, this is a circular argument.

Secondly, there is another argument against Beddor's claim that phonetically defined context is one of the factors involved in the phonological motivation of phonetic height shifts attested in mid vowels. Not only is it not possible to motivate phonetically whether a nasalisation phenomenon is contextual or distinctive: Even if Beddor could establish independently of phonetics which examples of phonetic nasalisation are not phonologically distinctive, the mere existence or presence of contextual (phonetic) nasalisation does not indicate any phonological relevance of this phonetic event. This question would have to be answered phonologically and, to avoid PH-induced circularity, independently of phonetics, *i.e.* in complete disregard of Beddor's phonetic data and within an approach mutually exclusive with hers.

Thirdly, Beddor's problems with the concept falsifiability become even more obvious when one compares the following two statements (parts of statement 1 in (16) have already been quoted above):

(16) *An example of Beddor's* [34] *circularity*

1. "However, it would be premature to conclude on the basis of first formant data [not corresponding to the observable vowel height shifts caused by nasalisation] that acoustic factors cannot account for phonological nasal vowel raising and lowering" [p. 238].

2. "The phonological generalizations that are not supported by the acoustic results are lowering of mid back non-contextual nasal vowels and lowering of high back nasal vowels. A closer look at the phonological data may shed some light on the lack of acoustic evidence of back nasal vowel lowering ... If back nasal vowel lowering were due to acoustic factors, or had other physical origins, we might expect back nasal vowels to lower irrespective of front nasal vowel shifts. Since back nasal vowel lowering is not independent of front vowel lowering, we might hypothesize that the physical origin of nasal vowel lowering is restricted to the front vowel system, and that back lowering is motivated by non-phonetic pressures such as pattern congruity (*e.g.* [ɛ̃ ɔ̃] rather

than [ɛ̃ ɔ̃ˆ]; see Martinet, 1955 [315])" [pp. 239f.].

Statement 1 presupposes that in Beddor's explanation phonological phenomena may be accounted for by acoustic factors. Statement 2, on the other hand, makes it clear that in Beddor's view acoustic evidence may be explained by phonological data ("phonological data may shed some light on the lack of acoustic evidence") and that, while some phonological phenomena (defined in phonetic terms) may be motivated by phonetics (*e.g.* phonological front vowel lowering), others (also defined in phonetic terms) may as well not be motivated by phonetics (*e.g.* phonological back lowering which may be "motivated by non-phonetic pressures"). More generally, phonology may be motivated by phonetics, it may as well not be and may or may not be motivated by non-phonetic factors. Phonetically defined (and thus assumed to be phonologically relevant) phenomena involving phonetically defined (and thus phonological) units may or may not be accounted for by phonetics. Note also that the circumstances under which phonology is or is not motivated by phonetics or some other 'evidence' are not established independently, *i.e.* scientifically, but in a way which Beddor might want to refer to as 'empirically'. In other words, phonetics or findings from other scientific disciplines motivate phonology when they do—and do not when they do not. The main disadvantage of such a view is that it is far too flexible and accomodating to be testable.

My fourth argument against Beddor's explanation is based on my observation that she has "no expectation for /u/ in the absence of significant centroid shifts" [p. 239]. Note that she finds that the "centroid data were not compatible with phonological /o/-lowering due to phonemic nasalization" [p. 249] and that there is "no consistent change for /u/ across languages" [*ibid.*], *i.e.*, without the gloss, "only the English centroid data were consistent with /u/-lowering due to nasalization" [*ibid.*]. So, in an empirical theory in which one wanted to use Beddor's assumption that centroid shifts are responsible for so-called nasality-induced 'phonological' vowel raising and lowering, the attested non-existence of significant centroid value changes from [u] to [ũ] would not, as Beddor seems to think, make no prediction but would predict that, universally, there is *no* change, *i.e.* neither raising nor lowering, from [u] to [ũ] (and thus, for her, from /u/ to /ũ/) and would not result in centroid data "not compatible with phonological /o/-lowering".

That Beddor is in need of an introductory lecture to very basic scientific notions (I recommend Magee [313] or Popper [392, 393, 394, 395]), becomes even more evident in Beddor [35]. In this article, Beddor admits that[34]

> "Of the models presented in the literature, neither those interpreted as generating default settings (e.g., Lindblom, 1983 [297], 1986 [298]; Westbury and Keating, 1986 [494]) nor those viewed as imposing physical limits (e.g., Ohala, 1981 [349], 1983 [350]) derive exceptionless predictions for phonological systems. It would appear that only constraints of the type 'the human vocal mechanism cannot produce the sound X' or 'the human auditory system cannot differentiate between the sounds X and Y' would yield such predictions. Yet to the extent that such constraints are known (see, e.g., Catford, 1977 [79]), they fall considerably short of characterizing ['phonologically'-non-phonetically] the vowel or consonant space utilized by the world's languages (Lindblom, 1983 [297], 1990 [299]; Ladefoged, 1985 [282])" (Beddor [35, p. 102]).

Clearly, Beddor has realised that phonetically motivated constraints fall *considerably* short of characterising phonologically (*not phonetically!*) defined concepts. Note that I agree totally! However, I do not understand at all how (on the following page) she can possibly come to the conclusion that "there is considerable [*sic*] potential for strengthening the contribution of phonetic models to phonological theory" [p. 103]. Before I am prepared to agree with this opinion, I would first like to see the evidence which would support an approach in which the same constraints which fall *considerably* short of being phonologically useful (in 1991) can still have *considerable* potential after decades of failure.

Note also that Beddor's problems become even worse if we consider what kind of data she bases her analysis on. So different authors have different judgements about the height status of OV and NVs of (presumably) the same language. Let me illustrate this with the Bengali and Hindi data Beddor uses. Pandey [362] concludes that in Hindi and Bengali "lower [oral *and* nasal] vowels were all raised and retracted, and higher ones were retracted, the raising being slight" [p. 218]. Ferguson & Chowdhury [159], on the other hand, write that "the [distinctive] nasal vowels [in Bengali] are generally somewhat higher than the corresponding oral vowels" [p. 81]. Kostić & Das [277], again, find that "the nasal vowels [in Bengali] are in the same articulatory position as the corresponding oral vowels" [p. 33]. Here Beddor [34, p. 129]

[34] In the following quote, I have substituted Beddor's square brackets '[ ]', within which she refers to other researchers, with parentheses '( )'; the square brackets employed contain thus, as elsewhere (when within a quote), references/comments made by myself, not Beddor.

follows Kostić & Das's findings because their data are based on spectographic data (and fit in), while Ferguson & Chowdhury's data are impressionistic (and do not fit in). When data misfits cannot be disgarded in this way, Beddor recognises them as "dialectal variation" [p. 81]. Apparently, Beddor's approach has been immunised against falsification.

For Hindi, Beddor [p. 98ff.] points to Fairbanks & Misra [156] for whom ẽː is lower than eː, to Kelkar [270, p. 24] who states that vowels followed by / ⁿ/ are slightly higher than other vowels, to Ohala [353, p. 26] who is of the opinion that, in general, vowels are lowered when nasalised, and to Kostić, Mittar & Rastogi [278, p. 42] according to whom the quality of Hindi NVs depends on tongue and lip position but is identical in comparable OVs. Beddor even states that

> "The position adopted here with respect to nasal vowel raising is that, for at least one dialect of Hindi, nasalization lowers mid front /eː/. Nasalization may also have a more pervasive (and possibly different) height effect in other dialects ..." [p. 100].

In Beddor's view, dependent on the language or dialect (presumably identical) articulatorily and/or acoustically defined properties of the speech production/recognition apparatus of the speaker/listener may or may not motivate phonology and may even differ with respect to how they do this. As long as the predictive power of Beddor's phonetically motivated approach remains non-existent, the empirical value of her findings are unclear. The problem with Beddor's research is not that the NVs found in a number of different languages—which are viewed as dialects or varieties of one and the same language by some—may exhibit different phonetic properties. The problem is that the assumption that the phonology involved is motivated phonetically can always be ignored when it does not work.

In addition to that, I would like to mention an article by Benguerel & Lafargue ('BL') [41] who, like Beddor, try to find a phonetic motivation for nasality-induced vowel height shifts.

BL take as their starting point that "the use of a certain phonetic dimension in a given language *may or may not* [emphasis mine] reflect a corresponding contrast in the phonology" [p. 309]. They point to the example of nasalisation, which, in both English and French, occurs phonetically in certain contexts but is phonologically contrastive only in French. They therefore want to establish how speakers are able to differentiate phonetic nasality which is a by-product of speech production from phonetic nasality which is "meant to signal a phonological contrast" [*ibid.*]. So let us be clear about this: For BL, there are two types of phonetico-scientifically established data; the type not motivating phonology (the by-product type) and the type motivating it (the type that means to signal the phonology).[35] The hypothesis they want to test is that there is a certain phonetically measurable nasality threshold, *i.e.* a "twilight" zone [p. 310], above which phonetic nasality does and below which it does not indicate phonological nasality. Unfortunately, they do, in my opinion, not address this matter in their article any further. They are merely able to find a *vague* correlation between degree of perceived nasalisation and velar height and between size of velopharyngeal cross-section and vowel height. In BL's view, there is

> "a definite relationship between the listeners' judgement of nasality and velar height. Results also suggest that a large velopharyngeal cross-section may be necessary for a French listener to perceive an open vowel such as [ɑ] as nasalized, whereas a smaller velopharyngeal cross-section may be sufficient to give a close vowel as [ɛ] and [ɔ] a nasalized quality" (BL [41, p. 309].

These findings do, however, not imply that the examples of correlation between the phonetically defined concepts observed and listeners' judgements are phonologically relevant. The phonetic correlates in question (*e.g.* larger velopharyngeal cross-section for [a] than for [ɛ ɔ]) may as well be a mere by-product of speech perception or production, *i.e.* part of the phonetic "packaging" (*cf.* Kaye [262, p. 210]) of a phonological, *i.e.* non-phonetically motivated, event. It can thus be said that BL not only fail to make a contribution to the answer to *their* question when phonetic nasalisation is phonologically significant (and when it is not); they also follow the HM blindly, without questioning the PH at any stage. Their research provides evidence neither for the PH nor for the HM.

There is a vast (phonetic) literature on the subject. I do, however, not have to continue here to discuss such researchers' work. All of the research I have looked at suffers from the same problem: it is circular or in some other way unfalsifiable, *i.e.* unrefutable. Phonetician-phonologists try to find out in what way and under which circumstances the phonetics of nasality can be derived or predicted from known phonological specifications, which, in turn, are already assumed to be phonetically

[35]Let us neglect here that I find it hard to find a scientifically coherent notion of phonetics within which phonetics motivates phonology—at least sometimes—and, simultaneously, signals it, *i.e.* is a signal of it.

motivated. The following quotes from Popper, with which I am in total agreement, sum up the problems the unfalsifiable status of the PH entails (*cf.* this volume, section 1.1 in chapter 1, pp. 16ff.):

> "Observations or experiments can be accepted as supporting a theory . . . only if these observations or experiments are severe tests of the theory . . . But testing a theory means trying to find its weak spots. It means trying to refute it. And a theory is testable only if it is (in principle) *refutable* [*ibid.*, p. 89].

Finally, as in the case of most 'phonological' manifestations of the PH, there are numerous examples where a language ignores the phonetic motivation to prefer nasalisation of low vowels. As pointed out in section 3.1.3, in approximately half of the languages exhibiting NVs, the set of NVs is a copy of the set of *all* OVs present in that system, not a copy only of the set of low OVs (*cf.* Hombert [222, pp. 359f.] referring to Crothers [121]). Let me now sum up the few examples where a language goes against the (supposedly) 'universal' trend of nasality-induced height shift. Hajek [196, pp. 123f.] points to the following cases: Firstly, according to Witucki [500], Chamorro high vowels are obligatorily phonologically nasalised when adjacent to /m/, while mid vowels are optionally and low vowels are never nasalised in this context. Secondly, in Akan only underlying high vowels, *i.e.* i ɩ u ʊ, undergo regressive nasalisation (with a following NC as trigger), but not mid or low vowels, *i.e.* e ɛ o ɔ a (*cf.* Schachter & Fromkin [430, p. 78ff.]).[36] Thirdly, in Vute only i and u, but not ɨ[37]e ə o a ɔ, are 'strongly nasalised' ("fortement nasalisés", [188, p. 31]) when enclosed by NCs (underlying nūùŋ 'poison pour flèche ("arrow-poison")' is realised [nū̃ù̃ŋ]). Similarly, in Gunu only i and u (not e ɛ a o ɔ) are nasalised when between two NCs, *e.g.* nũ̄ũ̀nè 'regarder ("to look at")' with nasalised ũ but nɛ̀nà 'garder (les enfants) ("to watch (the children)")' with oral ɛ (*cf.* Robinson [414, p. 54]).[38]

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[36]As mentioned in section 1.4.1.1, House & Stevens [225] measured vowels produced by an electrical voice tract analog and found that Formant 1 shifts up in nasalised vowels, but more so for [i] than for [a]. Based on this, Fant [157] and Ohala [347] claim that high vowels are easier to nasalise (perceptually) than low vowels. It is therefore possible that Schachter & Fromkin perceived nasalisation in mid and low vowels as weaker than in high vowels and therefore (motivated by phonetics) 'analysed' nasalisation in mid and low vowels as non-phonological. It may well be the case that regressive nasalisation (triggered by NCs) phonologically 'nasalises' all Akan vowels irrespective of vocalic height; this, however, requires a new analysis of Akan and probably more fieldwork.

[37]Hajek's source for Vute (*cf.* Hajek [196, p. 123]), *i.e.* Guarisma [188], has "ɯ" for ɨ, which, in her opinion, is an oral central (ɨ not back ɯ) "1er degré" ('high') vowel (*cf.* Guarisma [188, p. 31]).

[38]Guarisma and Robinson indicate nasality by a tilde underneath not above the vowel nasalised; for example, they have ṵ for ũ.

Fourthly, in Valaisan Franco-Provençal only reflexes of earlier iː uː have been affected by progressive nasalisation (*cf.* Bjerrome [48]). Fifthly, Flutre [165] reports spontaneous nasalisation in word-final stressed vowels only for the modern Picard developments of historical high vowels. Sixthly, in Panamanian Spanish, deletion of a syllable-final NC which follows a NV (as part of a (supposedly) 'sociolinguistic' spread of distinctive nasalisation) is strongly favoured in the case of high vowels (according to Cedergren & Sankoff [80]). Seventhly, in Chen's [91] sample of over 1200 Chinese dialects, there are 2 counter-examples to generalisation 1 in (5) (p. 110).[39]

To sum up, there are countless languages where the phonetic motivation for the HM does not exhibit any effect and a few examples where nasalisation prefers high as opposed to mid or low vowels. Neglecting the latter more marginal group, the existence of a trend (like height shifts co-occurring with nasality), which may well be describable in phonetic terms, does not imply in any way that this trend is phonetically motivated. On the contrary, if it were, one would expect this universal preference to actually be universal and not merely a tendency which can be ignored by half of the languages exhibiting NVs. I doubt, however, that an even higher frequency of such languages would make the mainstream drop the PH.

It is now clear that nasality-induced vowel height shifts are far less universal than phonetically-based universalists would like you to believe. It seems that the HM is a myth, nothing more. So apparently, there is no synchronic evidence for the claim that the phonetic process of vowel height adjustment in connection with nasalisation is of any phonological relevance. However, as I will show in chapter 6, it is the phonology which forms a relevant part of an explanation of (some of) the attested vowel height shifts and of cases of preferential nasalisation of low and/or mid vowels, not *vice versa*.

3.2.2 The irrelevance of synchronic 'morphophonemic' variation to phonological research

In this and the following section, I would like to point out two further problems typical for phonetically based synchronic evidence in general but also, more specifically, for evidence in favour of the HM: firstly, questionable assumptions about the phonological relevance of morphology (this section) and, secondly, about the motivation

[39]Hajek [196, p. 124] claims that reanalysis (not further specified) of some of Chen's data points to at least three more dialects which, contrary to Chen's generalisations, favour nasalisation of high vowels: Mengzi, Kaiyuan, Huaning. He suspects [p. 222, footnote 8] that there are many more exceptional dialects. Since he does not provide any evidence for this suspicion, I will ignore it.

for sound change phenomena (section 3.2.3). I will try to show that 'insights' gained on the basis of such assumptions cannot be used as evidence in an explanation of nasality-induced height shifts (or of any other non-universal, *i.e.* phonological and non-phonetic, processes).[40]

Let me start my discussion of the irrelevance of the notion 'morphological relatedness' to phonological research by pointing out that even though Beddor's [34] experiments are independent of morphological relatedness, BKG [36] findings are indeed based on such a concept. Since my criticism regarding irrelevant assumptions about morphology is not taken into account in any of Beddor's work, her conclusions in [34] and [36] are even more questionable than section 3.2.1 revealed. In other words, the relevance of her findings cannot be underestimated as long as the data (from other linguistic literature or her own tests) which she bases her generalisations on are not re-evalued.

As Kaye [259] has shown, morphologically related forms are not necessarily in any phonologically relevant relation, *i.e.* a relation where one form (or a part or parts thereof) is (are) phonologically derived from the other form (or a part or parts thereof); nor do morphologically related forms have to be phonologically derived from a common source. For example, BKG base their phonetic explanation of diachronic changes involving nasality on so-called "synchronic morphophonemic alternations [which] attest to historical lowering of high and mid vowels and raising of low vowels" [36, p. 198] in French. The examples they provide are [*ibid.*]:[41]

(17) *'Synchronic morphophonemic alternations' in French*

[iN] ~ [æ̃]	⟨fine/fin⟩	'thin (f./m.)'
[eN] ~ [æ̃]	⟨plénitude/plein⟩	'fullness/full'
[yN] ~ [œ̃]	⟨une/un⟩	'one (f./m.)'
[øN] ~ [œ̃]	⟨jeûne/(à) jeun⟩	'fast/fasting'
[aN] ~ [ɑ̃]	⟨planer/plan⟩	'to glide/level'

[40] An example in the linguistic literature where both synchronic morphological variation and sound change are 'captured' by the same rule-based theoretical apparatus is Posner [396]. Published in 1971, this article is mainly influenced by (then) recent literature about the chronology of the development of French NVs (*cf.* Haden & Bell [194] and Martinet [316]) and about the framework of generative phonology (*cf.* Chomsky & Halle's *SPE* [97] and Schane [433]). Present day phonology may, in places, have got over Posner's descriptive rules; however, due to the refusal on the part of the phonological mainstream to question the PH, most work called 'phonology' today, in line with Posner, still regards morphological and/or etymological relatedness as evidence for the proposal of phonetico-phonological 'connectedness', where 'connected' of course implies that it is the phonetics which motivates the phonology.

[41] 'N' in (17) "represents any nasal consonant" [36, p. 198].

Opposed to current views held by the phonological establishment, none of the forms in (17) are phonologically derived from a common source. There is no evidence which would suggest that the phonology of French takes into account that fin and fẽ ([fæ̃] for BKG) are morphologically related. Similarly, ⟨know-⟩ [nɒ-] in English ⟨knowledge⟩ is morphologically but not phonologically related to ⟨know⟩ [nəʊ], *i.e.* [nɒ-] (or a phonological representation thereof) is not phonologically derived from [nəʊ] (or a phonological representation thereof) nor *vice versa*. On the other hand, there is as little evidence for claiming that ⟨-ledge⟩ [-lɪʤ] in ⟨knowledge⟩ is in any synchronic morphological relation to ⟨ledge⟩ [lɛʤ] as there is for the proposal of any such phonological relation, *i.e.* [-lɪʤ] (or a phonological representation thereof) is not phonologically derived from [lɛʤ] (or a phonological representation thereof) nor *vice versa*. So while that which is pronounced [nɒ-] is morphologically related to the morphological representation of [nəʊ] and while the 'morphemes' corresponding to [-lɪʤ] and [lɛʤ] are morphologically unrelated, none of these forms are phonologically derived from any of the other three. [-lɪʤ] might sound more similar to [lɛʤ] than to [nəʊ], the phonological representation of [-lɪʤ] might even share more phonological units with [lɛʤ]'s than with [nəʊ]'s phonological representation, but this is no evidence for the claim that any of the forms mentioned or any phonological units therein are phonologically derived from another or from a common source.

On the contrary, I agree with Kaye [259] for whom phonology is a function ϕ[42] with a phonological string as its one argument. Phonological strings, *i.e.* the domains which the function ϕ is applied to, are (from the hearer's perspective) the addresses in the (hearer's) lexicon. In this view, different domains or addresses are not in a phonologically relevant relationship because they have certain parts of their phonological material in common. Since domains can be used to build larger domains (via the *concat*-function [p. 102]), it can be said that the phonology only 'cares' about how an acoustic, *i.e.* continuous, input string is to be chopped (parsed) into the correct phonological domains, *i.e.* addresses which can be looked up by the hearer to find out syntactic and semantic content of these addresses.[43]

[42]Or, due to the existence of optional processes, rather a family of functions, *i.e.* ϕ', ϕ'', ϕ''', *etc.* (*cf.* Kaye [259, p. 123, footnote 18]).

[43]Note that there is not even any necessary correlation between the 'freeness' or 'boundness' of a 'morpheme' (*cf.* Matthews [318, p. 160]) and the analyticity status of a domain. For example, o in the Turkish suffix ⟨-yor⟩ -jor∅ '*present continuous (3sg.)*' as in gelijo̲r 's/he is coming' does not undergo I-harmony ('palatalisation', to choose a descriptive, completely non-explanatory term) even though the nuclei in most Turkish suffixes do (*cf.* the -d∅-suffix '*past (3sg.)*' in geldi̲ 's/he came' or -maz∅ '*negative present habitual 3sg.*' in gelme̲z 's/he does not come'. The reason for this is that -jor∅ has its own domain while -d∅ or -maz∅ do not. So even though all of these suffixes are

Since it apparently makes no sense to assume that ẽ in fẽ is phonologically, *i.e.* synchronically, derived (via nasalisation) from i (on the basis of the occurrence of i in the morphologically related form fi̲n), BKG's attempt to find a phonetic explanation for the supposed derivation of ẽ from i is not empirically motivated. It is, however, interesting to see why the majority of phonologically interested researchers try to derive as many forms as possible from as few lexical ones as possible.[44] The reason for this is the commonly held belief that lexical storage is (cognitively) expensive. This in turn is due to the assumed nature of the human brain. It is hypothesised that the human brain is of a capacity so limited that an increase in lexical objects would incapacitate the storage and retrieval system as we know it or at least slow it down seriously. In this context, Jensen [240], working on a computational approach to the phonology of connected speech, provides the following two statements by Bromberger & Halle, on the one hand, and Chomsky, on the other:

> "... memory storage and search time are at a premium in the case of language" (Bromberger & Halle) [68, p. 56] (*cf.* Jensen [240, p. 16]).

> "I understand the lexicon in a rather traditional sense: as a list of 'exceptions', whatever does not follow from general principles" (Chomsky) [96, p. 235] (*cf.* Jensen [240, p. 17]).

As Jensen [pp. 15–18] points out, there is not only no evidence for the assumption that memory storage is at a premium, this hypothesis is also incompatible with assumptions about the theoretical construct called Language Acquisition Device ('LAD') which are made by the same theoreticians who also believe in minimisation of the number of assumed lexical objects. Jensen argues as follows: Firstly, there is no evidence which would suggest that there are linguistic constraints on the number or structure of idioms: "any linguistic structure can in principle be idiomatised" [p. 15]. So if we do not make an assumption expressing such a restriction, the LAD has to be set up in a way such that it "*must* behave as if (or it must assume that) it has enough resources available to it to list *any* subset of the set of linguistic structures" (Jensen

'bound', the phonology does not take this into account (Turkish data mine; the data is completely in line with Charette & Göksel [86, 87] or Lewis [294]). It might well be possible though that 'bound' and 'free' domains are stored in different parts of the lexicon. This is, however, irrelevant to the present discussion.

[44] I too used to share this view, which, in agreement with the arguments presented in Jensen [240], I now reject; *cf.* point 2 of the *Parsing Principle* which I proposed in [381, p. 79]: "No redundant information may be part of the lexicon".

[240, p. 16]). Therefore, the cardinality of $\mathfrak{L}$, *i.e.* the set of linguistic structures (as defined by a generative grammar), must be $\aleph_0$, *i.e.* countable infinity, and not a number that, because of whatever reason, has to be kept as small as possible. Secondly, the structural finiteness of the physical brain does in no way imply that the design of the LAD takes this finiteness or the finiteness of the physical lexicon into account. Thirdly, if the assumption that memory storage is expensive were a scientific one, it should be possible to set up an experiment

> "where the LAD is taken to and beyond its supposed limits. ... It would have to be demonstrated that an LAD could reach a point where it could no longer list a linguistic structure" [240, p. 17].

No such evidence exists.[45] More importantly, the available evidence is contrary to storage minimalism: Humans do not lose their ability to acquire new words once they have learnt a certain number of them. On the contrary, humans go on learning new idioms in their native language through their whole life, and even mature adults do not lose the ability to acquire other languages.

To sum up, it neither makes sense to derive one phonological domain (or parts thereof) from another (or parts thereof) because they are morphologically related—like allomorphs from the underlying morpheme—nor to subsequently try to find a phonetic explanation for this puzzling relatedness. Moreover, the motivation for this questionable approach, *i.e.* the belief that memory storage is costly, is totally unfounded. It is therefore an empirically uninteresting undertaking to establish phonetically motivated 'phonological' tendencies like nasality-induced vowel height shifts (which can, of course, entail any number of counterexamples) on the basis of so-called 'morphophonemic' alternations.

3.2.3 The irrelevance of diachronically related forms to phonology

In section 3.2.2 I have argued that morphologically related forms are not necessarily phonologically related, *i.e.* phonologically derived from a common source.

The common failure to differentiate morphological relatedness (however defined) from phonological derivation on the part of mainstream phonologists like BKG results in unfalsifiable phonetic 'explanations' of so-called 'phonological' non-phono-

[45] Regarding the lexicon and the human long (not short) term memory, let me quote one of my teachers, Jonathan Kaye [p.c.]: "Have you ever seen a human run out of memory?"

logical phenomena. This problem becomes even worse when phonological relatedness is established on the basis not only of synchronic morphological but also historical, *i.e.* etymological, relatedness:[46] Neither type of morphological relatedness between two forms A and B, *i.e.* neither synchronic nor diachronic (etymological) one, implies that A and B are phonologically derived from a common source.

Let us inspect the irrelevance of etymological relatedness to phonology more closely. As in the case of synchronic linguistics, the PH is *communis opinio* in historical linguistics. Sound change supposedly happens due to phonetic reasons (amongst others) which are thought to motivate historical and synchronic phonological phenomena alike (*cf.* Bynon [75], Jones [242] Kiparsky [274, 276], McMahon [322], Trask [479]); in other words, it is common practice to equate the motivation for historical processes with the motivation for phonological ones. In this view, perceptual and/or articulatory limitations of language speakers/hearers motivate (synchronic) phonological processes (resulting in 'allophony') and, when such processes become extinct, cause 'phonematisation' or lexicalisation of (some of) these allophones. So those phonetic processes which coincide with a phonetically motivated diachronic tendency are also assumed to be of the highest relevance to the establishment of those phonetic factors which are said to have an influence on synchronic processes and *vice versa*. For example, if it can be shown that French fẽ developed from °fin, this is taken to be evidence for the claim that, because of some phonetic motivation, nasalisation of ĩ lowers this vowel over time and, since phonetics is relevant to phonology, this is synchronically relevant. So the synchronic phonological representation of ĩ gets lowered phonologically because of phonetic reasons, which motivate both historical and synchronic phonological phenomena.

Now consider the following examples:[47]

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[46] *Cf.* Coleman [115], who, based on no arguments and as part of his supposedly synchronically relevant theory called 'declarative lexical phonology', tries to derive *e.g.* English ⟨church⟩ and ⟨kirk⟩, on the on hand, or ⟨drink⟩ and ⟨drench⟩, on the other, [p. 376] from a common source. Coleman's article [*ibid.*] is, by the way, wonderful teaching material: His 'attack' on GP is so devoid of arguments that it provides an excellent example for first-year students of how not to do science. For a discussion of Coleman's problems with basic scientific notions, *cf.* Kaye [259].

[47] In (18), as elsewhere, '⇔' stands for 'is etymologically related to', '⇎' for 'is not etymologically related to'.

(18) *French forms with no common phonological source*

a. *Etymologically related*

fin ⟨fine⟩ 'nice (f.)' ⇔ fẽ ⟨fin⟩ 'nice (m.)'
pinɛd ⟨pinède⟩ 'pinewood' ⇔ pẽ ⟨pin⟩ 'pine tree'
linje ⟨linier⟩ 'linen (adj.)' ⇔ lẽ ⟨lin⟩ 'linen (noun)'

b. *Etymologically unrelated*

fin ⟨fine⟩ 'nice (f.)' ⇎ fẽlãdɛ ⟨finlandais⟩ 'Finn (m.)'
pin ⟨pine⟩ 'cock, prick' ⇎ pẽ ⟨pain⟩ 'bread'
lin ⟨Lyne⟩ '*woman's name*' ⇎ lẽ ⟨lin⟩ 'linen (noun)'

There is no evidence in favour of the proposal that any of the forms in (18) are derived from a common phonological source. It is standard, however, to try to come up with a phonetic-'phonological' explanation for the etymological development of the pairs (left column—right column) in (18a.), but not in (18b.). This point of view does, however, not make any sense. If the nasalisation and lowering in ẽ in French ⟨pin⟩ pẽ is phonetically motivated, why did this not happen in the case of ⟨pine⟩ pin? The phonetic view can of course always be maintained by saying that the nasalisation and lowering process which applied to post- or late Latin °pi(:)n(u)- 'pine tree' (resulting in French pẽ) was executed at a time when what is now ⟨pine⟩ pin had either not been in usage yet or had not provided the phonological input necessary; *e.g.* n in some precursor of Modern French pin might not have been word-final yet, but nasalisation might only have been triggered by word-final NCs. This however, leaves unaddressed (as always) why speakers of different languages and/or of different times are in need of different degrees of the phonetically defined notion employed to explain these phenomena (like ease of articulation, perceptual distinctiveness, *etc.*): Why are the phonetic conditions for nasalisation dependent on time, language, speaker? In our example above, why would nasalisation of vowels by final NCs start or cease to operate in the first place? Did the evolution of the human brain make yet another leap (at least for some)? Did the the speech organs or some tissue inside the human ears change over time? Clearly, phonetics applies when it applies, is thus unfalsifiable and has no explanatory power in synchronic or historical linguistics.

One could try to counter my criticism of the established (and thus rarely questioned) view on the phonology-phonetics link by pointing to Ohala who states that

> "it will not be possible to say anything about individual languages or specific time periods in their history, simply that considering a large and representative sample of human languages, such-and-such confusion or change is more likely than another" [351, p. 268].

Even though, such an approach *could* be empirical, in Ohala's case the relative number of examples where the PH makes verifiable predictions can obviously not be low enough to make him drop the PH and the attested number will always do. On the contrary, Ohala is of the opinion that generative phonology has "gone too far" [348, p. 378] in a direction *away* from a "physical character to sound change" [*ibid.*]. So he objects to Postal's [397] view of sound change as a whim of fashion because this

> "would make it an amazing coincidence that so many different language communities over the ages and in distant lands experienced the same whims of fashion in pronunciation" [Ohala, *ibid.*].

Assuming that such 'whims of fashion' are not whims but are due to identical motivating factors, this does in no way imply that re-occurring sound change patterns are motivated by phonetics; as I have argued above, if they were, there should be far fewer languages where such powerful phonetic forces are ignored so consistently. Ohala, like any linguist adopting the PH does not test it, and unfortunately, this is not empirical.

In this context, it is important to discuss what phonological change over time *is* motivated by. I will show that the circumstances under which change phenomena occur are quite specific and that therefore there is no reason why a phonological theory should have to account for *Lautgesetze* ('sound (change) laws')—or any description of a diachronic change like the English vowel shift or the first or even second *germanische Lautverschiebung* (literally, 'Germanic sound shift')—as if they were (synchronic) phonological events. Opposed to the standard view, sound change finds its motivation in only two relevant factors: firstly, group mark changes and, secondly, languages-in-contact situations.

Let me start with group mark changes. Kaye [261, 262] has made a startling discovery which he sums up in the following way [262, p. 210]:

1. "Humans possess a group recognition system".

2. "Human group recognition cues are expressed through vocalism".

3. "The human vocal channel is shared between two discrete systems":

    (a) "The human linguistic system".

    (b) "The human group recognition system".

Kaye provides a number of arguments to support this position. Firstly, social animals, like humans, have means to distinguish group from non-group members; this distinction plays an important role in the selection of appropriate altruistic/cooperative behaviour and mate selection [*ibid.*]. Secondly, and more specifically, all primates have a group recognition system ('GRS') which enables them to recognise kin. Thirdly, "humans' 'phonetic' ability far exceeds linguistic requirements" [p. 211], *i.e.* all humans, including children, can recognise 'insiders' (group members) not previously known to them and 'outsiders' (non-group members) by their accent. Fourthly, Kaye points out that in primates, neocortex size correlates directly with group size. Since humans are the primates with the largest neocortex, he predicts that human groups are larger then those of other primates. This in turn increases the probability of humans having to recognise group members not previously encountered, which necessitates a GRS even more.

There is only one ability all humans share which enables them to recognise conspecifics (members of the same species) of their own group: recognising their own accent.[48]

Clearly, human group recognition appears to be based on phenotype matching, *i.e.* a system established via matching of cues given by conspecifics and assimilating these cues to form a single template [p. 212]. In the case of humans, the cues are, like the linguistic system, transmitted via the vocal channel. There the linguistic signal is mixed with vocal group stamp cues and other phonetic "packaging" [p. 210]. In this view, group recognition cues (determining the recognition template) are acquired together with the linguistic system. An individual can then compare this learnt template to the phenotype of a conspecific and determine whether or not the conspecific encountered is kin. Note that this makes any phonetically motivated approach to phonology futile. In line with the discussion in previous sections of this chapter and in chapters 1 and 2, there is some kind of vague link between phonology and

[48]This also applies, to a more limited extent, to the ability to distinguish different group marks, *i.e.* accents, of non-group members who are also non-group members to each other. For example, I am a native Augsburg Swabian speaker and, even when I am forced by the state from an early age on (school) to speak Standard German ('Hochdeutsch' as it is 'good' and 'proper') I have an Augsburg Swabian accent (group mark). I can always pick out Augsburgers; my ability to differentiate Bavarians from Austrians is, though existent, more limited.

phonetics, but it is never possible to establish conclusively on the basis of a phonetic description of a signal (containing linguistic *and other* information) which of the phonetic properties involved are phonologically relevant; this is always a 'phonological' matter.

Furthermore, since group marking is important for mate selection (*cf.* endogamy, exogamy), it follows from the above that this system can only work if the ability to acquire a group stamp is lost before mating. This is also why humans' capability to acquire an accent natively atrophies after puberty. Note that this is often confused with the ability to acquire a foreign language. Human *can* learn foreign languages for all of their life; it is the original accent which mature adults fail to assimilate successfully.

As discussed above, a phonetically motivated approach to historical linguistics cannot explain why human languages change nor does the existence of etymologically related forms imply that the development resulting in these forms has to be accounted for by phonology. Having looked at how closely accents resemble templates of a GRS, we are now able to understand why human languages must change. Groups (identifiable by a group stamp) grow and split into new groups, some of which might lose contact with each other. If the human GRS were static, after only few generations there could be many individuals sharing one and the same group stamp even though they would not be members of the same group anymore. Since groups are dynamic in nature, the GRS must be dynamic too. Remember that the vocal channel is shared by the GRS and the linguistic system. Since there is evidence that acoustic cues may be subject to phonological, *i.e.* linguistic, reinterpretation under certain circumstances (*cf.* below), linguistic change may be initiated on the basis of a group stamp change. In such a case, the group stamp change would 'spill over' into the phonology. Such spill-over is not motivated phonologically, and the resulting change does therefore not have to be accounted for by the phonology. If it were, processes not motivated by the phonology would be ascribed to it and our view on what constitutes a phonological process would become distorted.

Now it is clear that it follows from Kaye's assumptions that due to the changing nature of GRSs linguistic systems must change over time too. Apparently, Kaye is the first linguist to come up with a reasonable proposal about the inherent, *i.e.* non-sociolinguistic, cause of linguistic change.

Let me add here that linguistic change conditioned by a changing group stamp, *i.e.* change from within, needs a few generations to become noticeable.[49] If the change is initiated from outside, *i.e.* when the phonological system of a language is reinterpreted by mature non-group members, change will proceed much more quickly. This is, in my opinion, due to two reasons: acoustic cue overlap and source system reduction. I will start with acoustic cue overlap. Acoustic cues of phonological material may be phonologically reinterpreted. Based on phonologically established PEs, *i.e.* established independently of phonetics, it can be said that there is good evidence which suggests that English [æ] (as in ⟨bl<u>a</u>ck⟩) is the phonetic realisation of the phonological expression ('PE') (A); there is no I-element in this PE (*cf.* Kaye [262, p. 217].[50] In Standard German ('SG'), on the other hand, (A) sounds like (short) [a] (*cf.* Ploch [379]). English (A) [æ] is always reinterpreted as $(A \cdot I)$ by a native speaker of SG (but not of English), and $(A \cdot I)$ is realised as [ɛ]. So for a German the vowels in German ⟨f<u>e</u>tt⟩ 'fat' [ɛ] and English ⟨s<u>e</u>t⟩ [ɛ] and ⟨s<u>a</u>t⟩ [æ] are identical: [fɛt], [sɛt], [sɛt]. Obviously, even though each PE is directly interpretable at every level[51] and the interpretations of most PEs are distinct from each other, there is some overlap.

If a group of SG speakers were to reinterpret English in the manner described (thus creating English′), it would be the acoustic cue overlap that allows them to reanalyse the English vowel system instantaneously (usually without becoming conscious about it). Note that for the mature German speakers in our example (generation 1), this situation may only be a matter of reinterpreting one acoustic cue, *i.e.* short [æ] as $(A \cdot \underline{I})$ [ɛ], their children (generation 2), acquiring German next to English′, would not, as their parents did, interpret English′ based on a German parameter setting. Under such circumstances, the acoustic cues corresponding to the nuclear PEs in two versions of English′ (slightly different for generations 1 and 2) may only partly be based on identical PEs for both generations and may therefore be generated by quite different constraints.[52] A new language within one generation.

Even more drastic changes could occur when a group has to acquire a language that has PEs which do not occur in their native language. Whole series of PEs, *e.g.* (in a traditional categorisation) voiced stops or fricatives, may be reinterpreted, *i.e.* disappear, instantaneously.

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[49]Kaye's estimate is 3–4 generations [p. 214].

[50]For an introduction to GP's theory of elements, *cf.* chapter 4, section 4.1.2.

[51]This is due to the *Uniformity Condition* which states that "Phonological representations are directly interpretable at every level" (*cf.* Kaye [259, p. 92]).

[52]In section 4.1.4 (chapter 4), I give a brief introduction to *licensing constraints* (PE-generating constraints).

So while linguistic change from within operates slowly via accent change, *i.e.* via the gradual accumulation in the course of time of small linguistic changes instigated by the GRS, change in a language-in-contact situation can result in the rapid wholesale rearrangement of the phonology of a language. Importantly, in either case the phonology operates unmotivated by phonetics, and while sound change is dependent on the language-specific setting(s) of the phonology/phonologies involved, it is not motivated by the phonology but by the dynamic nature of the human GRS or by the inflexible nature of acoustic cue and parameter settings in mature humans.

Apparently, there is no point in establishing what we consider relevant to phonology on the basis of historical sound changes nor to explain such developments either phonologically or phonetically (and therefore phonetico-phonologically). A phonetic explanation predicts the 'phonological' convergence of human languages, which can not be supported by evidence. It also leaves unclear why some languages might develop slowly without changing much (*e.g.* German since the 16th century or Icelandic) while others change rapidly and radically (*e.g.* German between the 9th and 15th century). A view based on Kaye's proposals, on the other hand, is in accordance with these curious properties of 'sound' change: Without 'forces from outside', all languages change slowly and moderately due to spill-over from changing group stamps; in languages-in-contact situations, radical developments may occur.

Finally, it has become clear that sound change phenomena and etymological relatedness do not have to (and should not) be catered for by the phonology. As already pointed out in chapter 1, Dolbey & Hansson's [140] claim that sound change is mostly phonetically natural is meaningless. Furthermore, their proposal that the phonology should contain a historical module is based on some version of the PH, not on an empirical method as part of which Dolbey & Hansson would have to show in what way their approach competes with the view presented elsewhere or even here and why and/or in what respect it would win this competition.

In relation to my discussion of the HM, this means that phonology does not have to provide either a phonetic or a phonological explanation for the 'connection' between one oral form (fin) and a 'related' nasalised and height-adjusted form (fẽ) as long as there is no phonological evidence for doing so.

Conclusion

Let me now sum up the findings of this chapter. I have shown that there is no evidence for a conglomerate of related claims which I have labelled the 'Height Myth'. More specifically, phonologically relevant correlations between nasality and vowel height or between nasality and consonantal place of articulation (if observable at all) are not phonetically motivated. In addition, there is evidence which suggests that standard arguments in favour of such correlations as put forward by Pope in relation to the historic nasalisation and denasalisation in French or by Chen for a sample containing around 1200 Chinese dialects are flawed. Furthermore, since the non-existence of high NVs does in no way imply any historical or synchronic denasalisation phenomenon, languages with nasal but no high nasal vowels, do usually not point to the historical denasalisation of high vowels. Also, supposedly 'universal' nasality-induced vowel height shifts and the equally 'universal' preferential nasalisation of low and denasalisation of high vowels, as proposed by universalists like Pope, Chen, Ruhlen *etc.*, appear to be ignored by numerous languages. As I have shown, cases where vowel height shifts do co-occur with nasality are not phonetically motivated. More generally, it has become apparent that, contrary to views held by the phonological mainstream, there is neither historical nor synchronic evidence in favour of the phonological relevance of length, height, nasality or correlations between any of these phonetic concepts.

Finally, I have also provided evidence against the common practice to derive one phonological domain (or parts thereof) from another (or parts thereof) simply because they are morphologically and/or etymologically related or even to try to find a phonetic explanation for this puzzling relatedness.

Table 3.3: Nasalisation in Teke languages (examples)

	Proto-Bantu	*Fumu*	*Kṳkṳa*	*Ibali*	*Ndzi-ndziu*	*Ngu-ngwel*	*Gloss*
1	°-V₁ŋV₂						
2	°càŋgò			n-tsàà	n-tsàà		‘news’
3a.	°-unV₂					-wẽ(n)	
b.	°mànà			-mànà	-mànà		‘to finish’
4a.	°-unda					-ũũ(n)	
b.	°bándà			báánà	báánà		‘to begin’
5a.	°-uma					-ã(m)	
b.	°túmà	túmà	túmà	túmà	tóɔ̃̏		‘to send’
c.	°jámà	jámà		jámà	jɔ̃̏		‘to shout’
6a.	°bɪ́mbà	bíímà	bjîîmà	bíímà	bíɔ̃̏		‘to swell’
b.	°bʊ̀mbà	bvúúmà	bvwùùmà	bvùùmà	bvùɔ̃̏		‘to bake in ashes’
c.	°-imba					-ĩĩ(m)	
d.	°-umba °cúmbà	 súúmà	 fwúùmà	 fúúmà	 fúɔ̃̏	-ũũ(m)	 ‘to buy’
e.	°-emba °cémbò	-je(e)me	 n-tsj~éèmè ~eẽ	 n-tsíɔ̃̏	 n-ʧíɔ̃̏	-jɛ̃(m)	 ‘horn’
f.	°-omba °còmbà	-wo(o)mo	 fw~òòmò ~oõ	 fùɔ̃̏	 fùɔ̃̏	-ʊ̯õ(m)	 ‘to borrow’
g.	°-amba °ɟàmbé	 n-záámí	 n-z~ààmí ~aã	 n-zā̃ā̃	 n-zāɔ̃̄	-ãã(m)	 ‘god’

Table 3.4: Beddor’s generalisations about nasality-height correlation (on the basis of a summary by Beddor, Krakow & Goldstein [36])

<table>
<tr><th rowspan="2">V-Height</th><th colspan="3">Vocalic height adjustment if nasalisation is</th></tr>
<tr><th>distinctive</th><th colspan="2">contextual</th></tr>
<tr><td>HIGH</td><td colspan="3">lowering</td></tr>
<tr><td>LOW</td><td colspan="3">raising</td></tr>
<tr><td rowspan="2">MID</td><td rowspan="2">lowering</td><td>FRONT</td><td>raising — if corresponding back vowel is raised
lowering — otherwise</td></tr>
<tr><td>BACK</td><td>raising</td></tr>
</table>

Part II

The Cognitive Approach

Chapter 4

The Cognitive Solution: a 'Nasal' Element

In part 1 of this thesis, I have shown that neither the phonology behind phonetic 'nasality' or 'nasalisation' nor the co-occurrence of nasality and other phonetically defined events can be motivated phonetically. What I would like to propose in this chapter is that it is an *element, i.e.* a *cognitively* motivated unit, in combination with certain restrictions on its distribution at the (underlying) lexical and/or the (derived) phonological level, which is responsible for phenomena of this kind. The main advantage of this approach is that it avoids the high degree of circularity and unfalsifiability inherent in phonetically based mainstream frameworks.

I have organised this chapter in the following manner: First, in section 4.1, I introduce some of the basic notions employed by Government Phonology ('GP'), a cognitive phonological theory. This introduction is necessary because GP, the theory I will use to discuss phonological nasality, is, as a framework 'on the fringe', not easily accessible via convenient introductory manuals and textbooks. This summary of the theoretical background of my cognitive approach is split into the following sections. In section 4.1.1, I explain what assumptions GP makes in relation to the purposes of phonology. Opposed to the mainstream view that the phonology is motivated phonetically, I claim, partly based on Kaye [253, 258], that the phonology has mainly three functions: parsing, acquisition, and lexical addressing. Then, in section 4.1.2, I introduce the smallest melodic unit stipulated in GP: the element. I sum up the changes in Element Theory since the Revised Theory of Elements (the 'RET') in relation to consonants in section 4.1.3. Section 4.1.4 explains how GP employs generative constraints to predicts cross-linguistically attested vowel systems.

In the second and final part of this chapter (4.2), I discuss previous work on nasality within GP by Bamba [26], Bamba & Nikiema [27] and Tourville [477]; I point out what such approaches can explain, what questions they leave unanswered and how their findings argue against a phonetic but in favour of a cognitive explanation of phonological nasality. The main riddle still to be solved will be shown to be connected to the internal representation of the phonological expressions ('segments') involved and will ultimately be discussed in chapter 6.

4.1 Melody in Government Phonology

In order to be able to discuss a GP approach to the phonology behind the phonetic phenomena 'nasality' and 'nasalisation', I will in the following provide some background in relation to a cognitive view of phonology and a summary of the basic concepts of two subtheories within Government Phonology both of which relate to melody: the ToE (or Element Theory) and the Theory of Generative Constraints. Additionally, I will employ an analysis of Sakha and Yawelmani U-harmony which will provide evidence for two claims: Firstly, opposed to Cyran's [123] view, some governing relationships may be created at the phonological (*i.e.* derived) level of representation by defining a certain type of PE-generating constraints specifically for the lexical level. Secondly, and going against Backley & Takahashi's [23] definition of structure preservation ('SP'), melodic licensing relations established in the lexicon may be ignored at PL.

4.1.1 The cognitive view

GP does not assume a phonologically relevant phonetic level of representation. Phonology is seen as a purely cognitive function. Kaye [258] proposes the following two purposes of phonology:

(1) *The purposes of phonology (according to Kaye)*
The purposes of phonology are

1. to help the hearer parse the continuous input string into distinct cognitive units (the Parsing Hypothesis);

2. to provide a lexical addressing system (the Addressing Hypothesis).

Recently, some doubt has been cast on the relevance of purpose 1: Jensen [240] puts forward a cognitive phonological theory with a lexical level as the only level of representation. In this view there are no phonological processes, let alone those which might help the hearer parse. However, in a theory with only one lexical level, the idea can be maintained that parsing is a motivating factor for phonology. In this view, phonology would be seen not as a function but as the set of phonotactic constraints on the formal and/or substantive properties of a domain. Some of these restrictions would be universal, some language-specific. But even if we continue in line with standard GP to assume one lexical and one (derived) phonological level, there is no literature on how much a given phonological process actually helps the hearer parse or on why there can be languages with no or few phonological processes. In Ploch [377] I discuss the lack of phonological processes in Lio, a language spoken on Tanjung Bunga (Flores Island, Indonesia) and the apparent sparseness of such phenomena in many Austronesian languages (*e.g.* Lauje and Totoli (Himmelmann [218])). My claim is that languages with templates (*e.g.* Beijing Mandarin (Goh [174])) and/or simple prosodic structure need to provide the hearer with less support in the parsing of the input string than languages with more complex syllabic structure. This in turn seems to indicate that parsing is a phonologically relevant concept. However, in order to turn the Parsing Hypothesis into a falsifiable assumption, it must be established (cross-linguistically and in mathematical terms) how much parsing work the phonology actually does. Since this research has not been done yet, the Parsing Hypothesis needs to be treated with caution.

As I have proposed in an article on non-switch harmony in Yawelmani, Turkish and Sakha (*cf.* Ploch [383]), phonology has a third purpose: acquisition. In my view, identical acoustic cues may be motivated by different PEs. As a result, there are situations where a given set of acoustic cues, *e.g.* a vowel system, can be generated by more than one set of constraints (*cf.* section 4.1.4). Without phonology, *i.e.* without the help of phonological processes (or, in a world with only one level, of the corresponding lexical constraints), it would be impossible for the acquirer to decide on the correct set of constraints. In other words, certain language-specific sets of acoustic cues imply certain phonological processes.

The second hypothesis presented in (1) is the Addressing Hypothesis. It states that the phonology helps the hearer find the relevant lexical entries that a given utterance is made of. Within this approach, phonology is either seen as a function which is applied to the lexical form (Kaye, *cf.* above) or a module consisting (amongst other things) of well-formedness constraints on lexical entries (Jensen). If phonology is assumed to be a function (Kaye), there is only one level of derivation, the phonological level. Derivation is subject to the Minimalist Hypothesis [259, p. 91]:

(2) *The Minimalist Hypothesis*

Processes apply whenever the conditions that trigger them are satisfied.

In accordance with the Minimalist Hypothesis, (some) morphological structure is seen as consisting of units relevant to phonology in its function as addressing device (*cf.* Kaye [259]). So the phonology can interact with morphology only in two ways: phonology is either blind to the morphological structure of a domain, *e.g.* the phonology does not recognise any internal structure in the English domain [irrespective], or it can recognise that a domain is morphologically complex, *e.g.* English ⟨-man⟩$_1$ -mən in [[post]man$_1$] or ⟨-man⟩$_2$ -mæn in [[super][man$_2$]] and ⟨un-⟩ in [[un][reasonable]].[1]

To sum up, I would like to propose the following set of the purposes of or motivations for the phonological component of the Language Acquisition Device:

(3) *The purposes of phonology (according to Kaye and Ploch)*

The purposes of phonology are

1. to help the acquirer decide on the correct set of generative constraints (the Acquisition Hypothesis);

2. to provide a lexical addressing system (the Addressing Hypothesis);

   and (probably),

3. to help the hearer parse the continuous input string into distinct cognitive units (the Parsing Hypothesis);

For those unfamiliar with GP, let me add that it is important to keep in mind that, while in phonetically motivated approaches phonetically measurable differences between assumed segments may be used as justification for the proposal of the phonological relevance of these differences, in a cognitive view it must be shown independently of the phonetic contrast under investigation that this contrast is employed cross-linguistically as indicator of a phonological (natural) class. Note that phonetically based frameworks constantly refer to attested systems of contrast whenever the PH makes no verifiable or wrong predictions.

I have discussed this technique in much detail in part 1 of this thesis. Most recently, I discovered more examples: *cf.* Golston & Kehrein's [180] paper on the

[1]For a more detailed discussion of this point, *cf.* Kaye [259].

feature geometrical organisation of the laryngeal node (proposed to be dominated not by a node or segment but by a prosodic constituent) or Kehrein's [269] presentation on the elimination of (phonological-cognitive non-phonetically yet phonetically motivated) affricates. Also, Boursma [56, 57] assumes that phonetically motivated features (like [nasal]) and gestures (*e.g.* a velum gesture) in combination with the proposal of general assumptions about the properties of perception and human motor behaviour can explain phonological phenomena. He does not discuss that the phonetic categories he employs, *e.g.* the velum gesture, cannot define phonetic-phonological nasality as he refers to it, *e.g.* in "[ãmã] /ãmã/" [57, p. 15], *cf.* my discussion of Vaissière [482] in section 1.4.2. So Boursma's velum gesture is not a phonetically defined velum gesture but already contains some phonological (*i.e.* non-phonetically established) abstraction or analysis. Apparently, Boursma's phonetically based approach is like all versions of the PH set up in a way such that it deludes its supporters into thinking that their framework is phonetically motivated.

To use another example, Strazny [464] (working on tonal depression in Zulu) states that the hypothesis that "tonal depression is caused by a L tone associated with an obstruent (Laughren 1981) [287] is inconsistent with standard assumptions about the sonority of tone bearing units" [p. 1]. In his own words, one of the goals of his paper is to "determine the categorial status of depressor consonants, by characterizing them as a natural class on the basis of a phonetically defined feature" [*ibid.*]. I have provided an abundant amount of evidence against the PH in part 1 of this thesis. For our purposes here, it suffices to say that Strazny's view is flawed in the following way: The motivation of tonal depression may in Strazny's approach not go against the implications of the phonetically motivated concept 'sonority' which, as a phonetic concept, is rarely testable.[2] In Strazny's view, the main advantage of his approach is that it avoids the weakness of other analyses in that it does not mix articulatory motivation (for segmental features) with functional motivation (for tones) but, instead, represents tone in articulator-based terms [p. 17]. Even though Strazny does not discuss this, it is impossible for him (or other feature geometrists, *cf.* section 2.5) to decide solely on the basis of articulatory measurements which articulatory details are attested to be used for phonological constrasts and the definition of natural classes. Not only is it not possible to predict phonological nasality from measurements of velum height/movement or nasal flow rates: even if we assume the

[2]For an argument against an explanation of the shape of well-formed word-initial onsets in English motivated by reference to the sonority hierarchy, *cf.* Kaye [256, p. 301ff.].

relevance of velum height or nasal flow in relation to phonological nasality, it can also not be decided on the basis of articulatory measurements what *degree* of velum lowering or *amount* of nasal flow establishes phonological nasality. This has to be motivated functionally, *i.e.* by finding out which phonetically circumscribable strings are used for phonological contrasts and which ones are not.[3] So if Strazny wants to motivate segmental *and* tonal features articulatorily—presumably to improve the empirical basis of his theory—the question remains open why he does not attempt at any stage of his argumentation to refute his assumption that the well-attested matches between the phonetics and the phonology are due to a causal relationship from phonetics to the phonology? The answer is that Strazny believes in the PH and does therefore not question it.

A cognitive phonologist, on the other hand, is only interested in finding out whether the assumption that tonal depression is due to the presence of a cognitive unit makes falsifiable predictions about the phonological behaviour of phonological expressions (segments without inherent phonological timing) containing this unit or increases the truth content or decreases the falsity content of their explanation; whether or not such an analysis is in tune with 'sonority' or any other phonetic urge makes no difference.

4.1.2 The Theory of Elements

In (the latest revised version of) GP, there are no phonemes, *i.e.* units which contain melodic information along with a portion of time that is automatically (but not necessarily explicitly) assigned to them. On the contrary, melody is separate from timing. Timing is achieved by the assumption of the skeleton, *i.e.* a tier of timing units ('skeletal points'), which melodic units, *i.e.* so-called 'phonological expressions' ('PEs'), can be attached to (*cf.* KLV [267]). In short, PEs are the cognitive units, *i.e.* units the brain can manipulate, that can be found linked to the skeleton.

A PE, in turn, consists of at least one element, the smallest melodic unit employed in GP. Such elements are motivated cognitively, not phonetically. At present, there are the following five to ten elements: A/R I U L/N H/h (ʔ v).[4] PEs containing one

[3]This definition of 'functional' differs from Boursma's (*cf.* above, this section).

[4]How many and which elements are used, depends on the version of the ToE. Introductory literature on the ToE is Kaye [253, pp. 160ff.] and KLV [265]. In the version of the ToE employed in this thesis, the so-called *revised* version, R and A have merged into (new) A, H and h into (new) H, there is no empty element ('v' (as in KLV, [*ibid.*])) or '@' (as in Harris [200])); the merger of L and N will be discussed below. For an attack on the assumption of a ʔ-element, *cf.* Jensen [238].

element are called 'simplex', those containing more than one, 'complex'. Also note that each element may only occur once in an expression.

GP's explanation of the observable link between phonology and phonetics is that elements are thought to correspond to invariant acoustic cues[5] and thus have typical pronunciations; so A is usually realised as a- or r-type sound while there is no evidence for the phonological relevance of many of the phonetic differences catered for by the IPA ([a ɐ ɑ ɒ æ r ɾ ɽ ɽ̊ ɹ ɻ ʀ ʁ]).[6]

Such elements can be fused within phonological expression. Fused elements also have typical pronunciations; for example, A fused with I (dominated by a nucleus) always sounds like an e-type vowel; [e] (not [ɛ]) has, however, been shown to correspond to (A · I̲) *and* (I) in Okpe (Cobb [110, pp. 144ff.]), while (A · I̲) is pronounced [ɛj ∼ ej] in English (*b<u>ai</u>t*, *cf.* Kaye [262, p. 217]) and [e] in German (Ploch [379]); (I), realised as [e] in Okpe, sounds like [ɩ] in English (Kaye, [*ibid.*]) and German (neglecting certain Austrian varieties).[7] The important thing to keep in mind is that the semi-automatic and semi-conscious perception of sounds according to phonetically motivated 'established' and thus unquestioned distinctions seems to be more misleading than helpful.

In line with this view, Harris & Lindsey [204] come to the conclusion that

> "elemental patterns are not in themselves acoustic events. Rather they are to be understood as cognitive categories which are mappable in the first instance onto patterns in the acoustic signal. Viewed in these terms, articulation and perception are parasitic on this mapping relation. That is, elemental patterns constitute cognitive templates by reference to which listeners decode auditory input and speakers orchestrate and monitor their articulations" [204, p. 105].

Furthermore, elements within a PE are of one of two status types: head or operator. A PE may contain any number of operators and zero to one head(s). Elements co-occurring in one PE are, as explained above, fused (symbolised by the fusion op-

[5]Cf. Williams [496, pp. 40ff.], working on the phonological basis of speech recognition and referring to Stevens & Blumstein's [456] findings regarding such invariant cues.

[6]This is not to say that the phonetic differences between *e.g.* [a ɐ ɑ ɒ æ] may not be phonologically motivated. For example, in English (A̲) sounds like [ɑ ∼ a], (A) like [æ] (*cf.* Kaye [262]); in German, on the other hand, (A) is pronounced [a]. Also note that an A-element linked to a nuclear point is apparently realised as a-type sound, linked to an onset or (if rhymes are postulated) rhyme position as r-type sound.

[7]I am currently preparing an article about the vocalic systems of a number of varieties of German (Ploch [378]).

erator '·'). By convention, PEs are symbolised by parentheses, heads are underlined. A few examples: (U) and ($\underline{\text{U}}$) are simplex expressions containing the U-element. (U) is headless, ($\underline{\text{U}}$) is U-headed; (A · I), (I · $\underline{\text{A}}$) and (A · $\underline{\text{I}}$) are complex PEs containing A and I, (A · I) is empty-headed, (I · $\underline{\text{A}}$) is A-headed, (A · $\underline{\text{I}}$) I-headed.

The differences in head/operator ('H/O') assignment as discussed are used to motivate distinctions between PEs all of which contain the same elements. For example, in Standard German the long vowels eː and ɛː correspond to the PEs (A · $\underline{\text{I}}$) (eː) and (I · $\underline{\text{A}}$) (ɛː), short ɛ to (A · I) (*cf.* Ploch [379]). Note also that in Ploch's [*ibid.*] analysis for German (as in Kaye's [262] account for English vowels), differences in headedness are employed systematically: all long vowels are headed, while all short ones are headless. In addition, a phonological process may be specific to the H/O role of the element employed. Gibb [173, p. 115], for example, explains Finnish front-harmony as spreading of an I-*operator*, not of any I-element. So in her analysis I only spreads from y (I · $\underline{\text{U}}$), œ (A · I · $\underline{\text{U}}$), æ (I · $\underline{\text{A}}$) but not from I-headed i ($\underline{\text{I}}$) or e (A · $\underline{\text{I}}$). Differences in H/O role assignment are also employed for consonants. For example, Kaye [259] proposes that both voicing and nasality in voiced and nasal stops (respectively) is motivated by the presence of an L-element. Based on his view that constituent governors, *e.g.* heads of branching onsets, must be headed while non-governors may be headless, he assumes that voiced stops (which are well-formed onset heads, *cf.* English ⟨grain, blame, drain⟩) contain L as head (b (U · ʔ · $\underline{\text{L}}$)) while nasal stops (which are bad constituent governors, *cf.* English *#mr. . . , *#ml. . .) are headless and contain L as operator (m (U · ʔ · L)). Let me add that I will not introduce more details regarding elemental representations for consonants here but will do so at the appropriate places in section 4.1.3 (this chapter) and in chapter 7.

Another notion important to understand the make-up of PEs, is the concept 'licensing'. More specifically, the head of a PE is said to license its operator(s). So in (A · $\underline{\text{I}}$), an I-head licenses an A-operator, in (A · I · $\underline{\text{U}}$), an U-head licenses an A- and an I-operator; in (I) and (A · U), on the other hand, there are only operators, none of which is licensed. How licensing is used to generate language-specific sets of PEs, I will explain in section 4.1.4.

Furthermore, opposed to Harris (*e.g.* [200]) who proposes a version of Element Theory in which all PEs are headed, there are no dummy-headed expressions in the version employed here: Headless expressions here correspond to dummy-headed expressions, *i.e.* PEs headed by the dummy or 'empty' element ('@') in Harris's framework.

Finally, if a skeletal point does not dominate any melodic material, it does not dominate an empty expression but simply no expression; in such cases, there is an empty point (not an empty expression).[8]

Let me sum up the constraints on the shape of PEs as discussed above:[9]

(4) *Constraints on a PE*

1. A PE contains an improper subset of the set of elements {A I U N LH (?)}.[10]
2. Elements within a PE are fused.
3. An expression has at most one head, and zero or more operators.
4. A head licenses its operator(s).

Having looked at how the ToE deals with vocalic expressions, I will sum up in section 4.1.3 the most important assumptions regarding consonants made by the version of the ToE employed in this thesis.

4.1.3 Element Theory: consonants

Based on an idea originally proposed by Vergnaud [483] and akin to other A-I-U phonologies (*e.g.* Anderson & Jones's Dependency Phonology [8] and Schane's Particle Phonology [434]), KLV [265] started Element Theory. Let me first sum up what elements were proposed until 1993/1994 and how they were used by Heo [212] in his thesis on Korean phonology. After that, I will illustrate how the revised version I will base this thesis on would handle the relevant data. This revised version is mainly based on Kaye [258], and I will from now refer to it as 'Revised (version of) Element Theory' ('RET'). Since there is virtually no literature on the how the RET accounts for consonants, this will provide an appropriate introduction. Another reason for why this section is necessary is that I will show that there are two ways how in the RET

[8]Such empty points are subject to the Empty Category Principle (*cf.* Charette [84], Gussmann & Kaye [190], Kaye [255, 256, 259]). Note also that GP stipulates that only constituents dominating a skeletal point may be licensors. Since onsets are licensed by the following nucleus (which can and must license this onset to its left), nuclei must dominate a point, non-licensing onsets may or may not dominate a point; *cf.* Charette [83] for a discussion of French onsets. However, onset heads which govern (and thus license) a preceding coda position or, within a branching onset, a (following) recessive point must, since they are licensors in such environments, dominate a point.

[9]This is a much altered version of Williams's [496, p. 95] formulation.

[10]This constraint stipulates that each expression contains at least one of the elements employed, each of which may only appear once in an expression.

an H-head can be linked to an onset point: fused with the rest of the melodic material associated with that onset point or unfused with it. This distinction between fused and unfused (extrapolated) H-heads will become relevant in my discussion of the merger of L and N (chapter 5) and my analysis of postnasal hardening in Bantu languages (chapter 7).

First, I will list the elements employed by Heo for consonants and their salient properties [*ibid.*, p. 89]:[11]

(5) *Elements for consonants (pre-RET)*

U°	labial	I°	palatal
v°	none	R°	coronal
ʔ°	occluded	h°	narrowed
N^{+}	nasal	H^{-}	stiff vocal cords
L^{-}	slack vocal cords		

In Element Theory (including the RET), the same PEs which can be linked to nuclear points (with the perceptual result 'vowel') can also be dominated by onsets or onset licensees (codas). For example, the PE (U) is pronounced [ʊ] when associated to a nucleus but [w] when linked to onsets or postnuclear rhymal positions. However, some elements, *i.e.* ʔ°, R° and h° can only be linked to consonantal positions.

To explain the main concepts of the RET in relation to consonants, let me sum up the lexical PEs Heo [212] proposes for Korean (6α.). Even though no one has, to my knowledge, provided an update of Heo's analysis within the RET, I will add the RET versions of the PEs given by Heo (6β.):[12]

[11]For more details and evidence for this set of elements, *cf.* Harris [198]. Note that the ATR-element which was still in use in Heo's version of Element Theory could not be attached to onsets and postnuclear rhymal positions ('codas') because of which it is not listed in (5). Also, the salient properties presented in (5) have little theoretical value. In agreement with a view outlined in Williams [496], I assume that elements (and consequently PEs) have acoustic correlates which in turn are targets for articulation. I add that I proposed in Ploch [383] that this correlation is vague enough for certain acoustic cues being motivated by sometimes up to three PEs (*cf.* section 4.1.2 below). One of the motivations for phonology is the disambiguation necessary due to this situation (the Acquisitional Hypothesis, *cf.* (3)).

[12]Heo's 'c' is represented by 'ʧ' here. p° t° k° ʧ° s° are called 'neutral' in Heo [212] because, together with m n ŋ r h they form the class of neutrally charmed segments [*ibid.*, p. 90]. Even though I do not use charm theory here I have kept the term 'neutral' for (neutrally charmed) obstruents in order to differentiate them terminologically from the tensed and the aspirated obstruents. In certain contexts, r changes to l which does not occur in the lexicon (*cf.* Heo [*ibid.*]). Also, there is no s^h in Korean.

(6) *The lexical internal representations of Korean consonants*

a. neutral PEs

	α. *Heo*	β. *RET*
p°	$(ʔ^{\circ} \cdot h^{\circ} \cdot \underline{U}^{\circ})$	$(ʔ \cdot U)$
t°	$(ʔ^{\circ} \cdot h^{\circ} \cdot \underline{R}^{\circ})$	$(ʔ \cdot A)$
k°	$(ʔ^{\circ} \cdot h^{\circ} \cdot \underline{v}^{\circ})$	$(ʔ)$
ʧ°	$(ʔ^{\circ} \cdot h^{\circ} \cdot I^{\circ} \cdot \underline{R}^{\circ})$	$(ʔ \cdot I \cdot A)$
s°	$(h^{\circ} \cdot \underline{R}^{\circ})$	$(H \cdot A)$
m	$(ʔ^{\circ} \cdot N^{+} \cdot L^{-} \cdot \underline{U}^{\circ})$	$(ʔ \cdot L \cdot U)$
n	$(ʔ^{\circ} \cdot N^{+} \cdot L^{-} \cdot \underline{R}^{\circ})$	$(ʔ \cdot L \cdot A)$
ŋ	$(ʔ^{\circ} \cdot N^{+} \cdot L^{-} \cdot \underline{v}^{\circ})$	$(ʔ \cdot L)$
r	$(\underline{R}^{\circ})$	(A)
h	$(\underline{h}^{\circ})$	(H)

b. tensed PEs

	α. *Heo*	β. *RET*
p'	$(ʔ^{\circ} \cdot h^{\circ} \cdot H^{-} \cdot \underline{U}^{\circ})$	$(ʔ \cdot U \cdot \underline{H})$
t'	$(ʔ^{\circ} \cdot h^{\circ} \cdot H^{-} \cdot \underline{R}^{\circ})$	$(ʔ \cdot A \cdot \underline{H})$
k'	$(ʔ^{\circ} \cdot h^{\circ} \cdot H^{-} \cdot \underline{v}^{\circ})$	$(ʔ \cdot \underline{H})$
ʧ'	$(ʔ^{\circ} \cdot h^{\circ} \cdot H^{-} \cdot I^{\circ} \cdot \underline{R}^{\circ})$	$(ʔ \cdot I \cdot A \cdot \underline{H})$
s'	$(h^{\circ} \cdot H^{-} \cdot \underline{R}^{\circ})$	$(A \cdot \underline{H})$

c. aspirated PEs

	α. *Heo*	β. *RET*
p^h	$(ʔ^{\circ} \cdot h^{\circ} \cdot H^{-} \cdot \underline{U}^{\circ})(h^{\circ})$	$(ʔ \cdot U)(\underline{H})$
t^h	$(ʔ^{\circ} \cdot h^{\circ} \cdot H^{-} \cdot \underline{R}^{\circ})(h^{\circ})$	$(ʔ \cdot A)(\underline{H})$
k^h	$(ʔ^{\circ} \cdot h^{\circ} \cdot H^{-} \cdot \underline{v}^{\circ})(h^{\circ})$	$(ʔ)(\underline{H})$
$ʧ^h$	$(ʔ^{\circ} \cdot h^{\circ} \cdot H^{-} \cdot I^{\circ} \cdot \underline{R}^{\circ})(h^{\circ})$	$(ʔ \cdot I \cdot A)(\underline{H})$

As can be elicited from a comparison of the α- and β-columns, charm has been eliminated in the RET. H^{-} and h° have been merged into (new) H, A^{+} and R° into A. Note that the RET presupposes the merger of pre-RET L^{-} and N^{+} into (new) L.[13]

The result that we get is as follows. The RET has the following six elements: A, I, U, L, N and ʔ. Let us also stipulate that ʔ can only be linked to onsets and onset-licensees, *i.e.* postnuclear rhymal positions ('codas'). h° cooccurring with H^{-}

[13] I will discuss the merger of L and N in more detail in chapter 5.

in Heo's version corresponds to the presence of an H-head in the RET. Both ʔ° or the result of the fusion of h° and ʔ° in Heo's analysis are simply motivated by ʔ in the RET. Even though this cannot be predicted in this system without an additional stipulation, both L and H can only co-occur with ʔ if L or H are head.[14] Note that the empty element v has been dropped in the RET. This means that while in Heo's ToE there are no headless expressions, there are in the RET: in general, v°-headed PEs in Heo's explanation are empty-headed (headless) PEs in the RET.[15] While in Heo's work elements other than H^- or L^- are often employed as heads of PEs, in the RET A, I, U are in general not head when they are fused with L or H; nobody working within the RET has to my knowledge found evidence for the assumption of ʔ-headed PEs. Since the neutral PEs in (6a.) are ill-formed inter-onset governors in Korean—because of which Heo assumes them to be charmless—, they are headless in the RET; well-formed inter-onset governors are negatively charmed for Heo, headed for me in the re-interpretation given here.[16] Furthermore, the presence of h° without H^- in pre-RET versions of the ToE is represented by H in the RET. Finally, the PEs in (6c.) are meant to represent light diphthongs, *i.e.* two PEs linked to one (onset) point without any phonologically meaningful ordering between them. For example, $\mathrm{p^h}$ is for Heo motivated by two PEs linked to one onset point in a way such that h° is shared by both of these two PEs. In the RET, on the other hand, there is no evidence to assume that any part of the stipulated light diphthongs is shared by both PEs. Thus Heo has $(ʔ^\circ \cdot \mathrm{h}^\circ \cdot \mathrm{H}^- \cdot \underline{\mathrm{U}}^\circ)(\mathrm{h}^\circ)$ for $\mathrm{p^h}$, the RET has $(ʔ \cdot \mathrm{U})(\underline{\mathrm{H}})$.

Let me now briefly show how the RET (in comparison to Heo's analysis) can still account for the Korean facts without assuming charm or the elements h, R and N. As already pointed out, there is onset-to-onset government in Korean. More specifically, right-to-left onset-to-onset government is one of the conditions on p-licensing in Korean (for details, examples and references, *cf.* chapter 5). Charmless (Heo) or empty-headed (RET) PEs may not be inter-onset governors. More evidence for the relevance of these categories (charmless, headless) can be found by looking at domain-final neutralisation. In Korean, only charmless (or headless) PEs may be dominated by an onset which is licensed by a domain-final (p-licensed) empty nucleus; such an onset is also not licensed to be linked to a PE containing H^-, h°

[14]There is unfortunately not enough space here to discuss such stipulations.

[15]The term 'empty-headed' is pre-RET and RET; it means 'v°-headed' in pre-RET frameworks, 'headless' in the RET. Harris (*e.g.* [200]) and Harris & Lindsey [204, 205] still assume an empty-element ('@' for them) but have eliminated charm. So 'empty-headed' means '@-headed' for them.

[16]I discuss the governing properties of Korean consonants in some more detail in chapter 5, *cf.* (8), (15) on pp. 198, 209 (respecitvely).

or I° (Heo), *i.e.* H or I (RET). Also, all onsets in such an environment must contain ʔ° (ʔ). For example, ʧ°uk°i 'porridge *(nom.)*' has as citation form ʧ°uk°, pak'e 'at the outside' has pak°, puəkʰe 'at a kitchen' has puek°. The resulting neutralisation pattern can be summed up like this (*cf.* Heo [212, p. 83]):

(7) *Korean consonantal neutralisation preceding a final p-licensed empty nucleus*

Underlying form	k°, k', kʰ	p°, p', pʰ	t°, t', tʰ	ʧ°, ʧ', ʧʰ	s°, s'	r
Phonological form	k°	p°	t°			l

So we see that headedness (or headlessness) is a useful tool in an explanation of the apparent restriction on Korean inter-onset licensees and of domain-final neutralisation.

Importantly, inter-onset government can provide us with even more information about the internal make-up of Korean consonants. However, let me first sum up the restrictions on inter-onset licensing. In a sequence of two onset-nucleus pairs ($O_1N_1O_2N_2$), where N_1 is empty, N_1 is licensed to be silent if it is p-licensed (*cf.* Charette [82, 83], KLV [267]).[17] Domain-internal (*i.e.* non-final) empty nuclei can only be p-licensed in Korean via proper government (which is, universally, head-final). N_1 is properly governed if firstly, the potential proper governor α and the potential proper governee β are adjacent at the nuclear projection (P_N); N_2 (α) and N_1 (β) are adjacent at P_N. Secondly, α (N_2) must not be itself licensed; if N_2 dominates a PE, *i.e.* if it is not empty, is is not itself licensed and thus—if all other conditions are met—a well-formed proper governor. Thirdly,[18] the onset licensees of α and β must be in an inter-onset government relationship. In Korean, this relationship is head-final; in other words, in Korean the third condition on p-licensing is that O_2 must be able to inter-onset govern O_1.

Relevant here is that O_1 must, firstly, dominate a PE containing ʔ° (or ʔ); so r or s° are ill-formed in this position if N_1 is silent. Secondly, PEs containing H^- (or H) are bad inter-onset licensees; consequently, all tensed and aspirated consonants may not be onset-to-onset governed. Furthermore, PEs containing H (Heo) or an H-head (RET), *i.e.* tensed or aspirated consonants, are well-formed inter-onset licen-

[17]Note that the leftmost nucleus in a domain cannot be p-licensed (silent) in Korean (and some varieties of French (*cf.* Charette [83]) and in Tonkawa (*cf.* Yoshida [512]). This means that the index numbers in $O_1N_1O_2N_2$ are to be understood as positional variables but not as fixed positions in a domain and could, with p-licensed N_1, in absolute terms and counting from the left, be filled by $O_2N_2O_3N_3$, $O_3N_3O_4N_4$, $O_4N_4O_5N_5$, *etc.*, but not by $O_1N_1O_2N_2$.

[18]I neglect one condition on proper government here: α may not be a government licensor. This condition plays no role in Korean.

sors. Since both tensed and aspirated consonants must contain an H-head in the RET, we must assume that there are two ways how an H-head can be linked to an onset point. This justifies the distinction between those PEs which exhibit an H-head fused with the rest of the melodic material linked to the onset point in question (tensed consonants) and those which have an unfused H-head (aspirated consonants).

Finally, I have to introduce how the RET uses the L-element to account for voiced and nasal stops. As I will discuss in more detail in section 4.2 and, in particular, in chapter 5, Kaye [258] proposes to merge pre-RET N (motivating consonantal and vocalic nasality) and L (motivating low tone/pitch on vowels and some instances of consonantal voicing). So the RET already assumes only one 'merged' element (new) L. For my discussion of consonants it suffices to say that in the RET, voicing in stops and fricatives if motivated by an L-head linked to an onset or an onset licensee;[19] examples are: French b denotes the acoustic target motivated by (ʔ · U · $\underline{\text{L}}$), v corresponds to (U · $\underline{\text{L}}$). Note that 'b' is employed for a variety of acoustic targets; so b in French—and Romance languages in general—denotes a voiced stop (often prenasalised), b in English is usually a neutral stop, *i.e.* neither voiced nor aspirated. In addition to that and, as I will show for vowels in section 4.1.4, identical acoustic cues may be motivated in different languages by different PEs. In other words, the fact that in a certain transcription a certain sound is transcribed as b does not indicate that this sound is voiced; and even if it is voiced, one still must take into account that b may be motivated by a range of PEs and that its internal representation must be established on the basis of an analysis of the system of contrasts employed by a language and the processes operative in it. Another example of the misleading nature of phonetics can be found in the study of the phonological behaviour of v: while French v may be a constituent governor (⟨vrai⟩ vrɛ 'true'), (High) German v may not be (*#vl, *#vr).[20] Apparently, French v is motivated by (U · $\underline{\text{L}}$), German v by an expression not containing an L-head.

To finish the introduction to the usages of L according to the RET, let me add that a consonantal L-operator is phonetically realised as nasality; for example: m

[19]Note that L-heads dominated by an onset licensee—*i.e.* another onset (which would be an inter-onset licensee) or a postnuclear rhymal position ('coda')—appear to ill-formed in many languages. For example, while in English zm (opposed to sm) is rare (*cf.* Kaye [256, p. 312n]), it is ill-formed in German (in an updated RET-version of Brockhaus [66]) or French (Kaye [256, pp. 308f.]).

[20]There are only very few examples of domain-initial vr in German, there are no cases of initial vl; *cf.* ⟨Wrack⟩ vrak 'wreck', ⟨wringen⟩ vrɪŋən 'to wring'. These appear to be borrowing from Low German. I have no explanation for why vl is ungrammatical in German.

(ʔ · U · L).[21] (8) contains a summary of the phonetic realisations of (new) L:

(8) *The phonetic realisations of (new)* L *(RET)*

	in nuclei	*in onsets and onset licensees*
L-*operator*	low tone/pitch	nasal stop
L-*head*	vocalic nasality	voicing in stops/fricatives, prenasalisation in voiced stops

In this section, I have introduced the main stipulations the RET makes in relation to consonants. Additionly, I have shown that there evidence independent of nasalisation which suggests that there the RET needs to stipulate a differentiation between fused and extrapolated H-heads.

4.1.4 The Theory of Generative Constraints

GP employs so-called 'licensing constraints' ('LCs')—proposed by Kaye [258]—to generate language-specific sets of PEs from the universal set.[22] Since then (1993), a whole series of LC types has been proposed. The summary in (9), based on Ploch [383], will clarify this:

(9) *Types of generative constraints (default:* OFF*)*

1. Total or partial exclusion of fusion

   (a) Elements may fuse. (OFF in Totonaco (Aschmann [20], (Classical) Arabic; ON in Yawelmani, English, Finnish, Turkish, German, French)

   (b) I and U may fuse. (OFF in Yawelmani, English; ON in Finnish, Turkish, German, French)

2. PE constraints

   (a) PEs must be headed. (H/O constraint)

   (b) Operators must be licensed. (LC)

[21] In section 5.2.4.1 (chapter 5), I will provide evidence for the presence of a ʔ-element in nasal stops.

[22] *Cf.* Cobb [110, pp. 46ff.] for further background information on LCs. Note that virtually all proposed generative constraints relate to nuclear expressions; there is little known about how to generate consonants systems.

3. H/O constraints on elements
    (a) Element X must be head.
    (b) Elements X must not be head.
4. LCs on elements
    (a) Element X must not be licensed.
    (b) Element X must not be a licensor.

Cobb [110, pp. 63ff.] discusses a few more GC types ('LCs' for her), *e.g.* 'Elements cannot be heads' or 'Heads license no operators'. Which GC types are relevant can, at present, not be arrived at independently and must therefore be established the (Sherlock) Holmes-Popper way: by radical (possibly counter-intuitive) elimination of all falsifiable assumptions whose verisimilitude is lower than of competing assumptions. Importantly, this notion of empiry does not make any assumption in GP unfalsifiable; GP *does not make any claim* in relation to which types of GCs are part of Universal Grammar; this is the topic of an ongoing investigation. Opposed to that, the view that phonetics motivates phonology *is* a claim—which, in combination with the assumption that it can be established 'empirically' under which circumstances it actually does so, ensures that the PH can never be wrong. Note also that, as initiated in Ploch [383], I use the general term 'generative constraint' ('GC') for Kaye's 'LCs', 'LC' for constraints on the licensing powers/status of PEs and elements, and 'H/O constraint' for restrictions on the H/O properties of PEs and H/O status of elements. Restrictions on the shape of PEs, *i.e.* on all heads or all operators of all PEs in a language, I refer to as 'PE constraints'.

Let me provide a few examples. In some languages—*e.g.* in Totonaco (Aschmann [20]), Moroccan Arabic, Aleut, Alaskan Eskimo, Amuesha, Jaqaru (Crothers [121], Lass [286, p. 142]) and Cree (Hockett [220]), only three vowels are well-formed (neglecting possible phonologically relevant length distinctions). Such a vowel system invariably consists of a i u. We can explain this by the assumption of the three elements A, I, and U and the parameterised constraint 1a in (9).[23]

[23] In Alaskan Eskimo, we find ä for a, in Amuesha e o for i u (respectively), in Jaqaru ä ɯ for a u. Since it is the phonology which corresponds roughly to invariant acoustic cues—without the latter motivating the phonology—, since all of these deviations are phonetically close and since there is to my knowledge no phonological evidence which would suggest that these phonetic differences are phonologically relevant, I claim that all of these languages exhibit the default setting (OFF) of the parameterised GC "Elements may fuse".

Languages like Finnish and Turkish allow y- [y ʏ] and ø-type [ø œ] vowels. English and Yawelmani, on the other hand, do not.[24] Since there is evidence from Finnish, Turkish, German and French that y-type vowels correspond to PEs containing I and U while ø-type vowels reflect PEs made up of A, I and U, it can be said that the languages of this world are divided into those where I/U fusion is allowed (Finnish *etc.*) and those where it is not (English *etc.*). The GC 1b, which is only relevant when GC 1a is ON, expresses this differentiation. Let me add here that Kaye [260] tries to explain the non-occurrence of I/U-fusion by the proposal of 'natural lexical heads'. This is to say that, if no language-specific constraint alters this, I and U (but not A) can only occur as head of a PE. Since PEs can only have one head, I/U fusion but not I/A or U/A fusion would be ungrammatical by default in this account. In my opinion, the main disadvantage of this approach is that many languages which do not display I/U fusion and in which both I and U would thus by default be natural lexical heads, *do* exhibit lexical PEs in which either I or U is not head. For example, in Kaye's [262] account of English (without y- or ø-type vowels), short ɩ (pɩt) and ʊ (pʊt) correspond to the PEs (I) and (U) (respectively). Apparently, the proposal of natural lexical heads has not been researched well enough yet. I therefore prefer an obvious stipulation specific to I/U fusion as in GC 1b.[25] Note also that I see all GCs as default settings. Opposed to Government and Binding-type syntax, where parameters (usually) have no default setting and where the acquirer must decide which way the parameter in question is to be switched (*cf.* Haegeman [195, p. 12ff.] on parametric word order variation), I assume that, without evidence to the contrary, the acquirer will not assume that elements fuse, and, if they do, the acquirer will by default not assume that, *e.g.*, I and U fuse, or that A is not a licensor, *etc.*.

There are two types of PE constraints which can be supported by evidence: 'PEs must be headed' and 'Operators must be licensed'.[26] The former is self-explanatory. A language that employs this restriction does not allow headless PEs; in other words, all seven universally possible empty-headed expressions, *i.e.* (I), (A), (U), (A · I), (A · U), (U · I), (A · U · I), are ungrammatical in such languages. An example

[24]Regarding Finnish, *cf.* Gibb [173]; Turkish, Charette & Göksel ('CG') [86, 87], Lewis [294]; German, Ploch [379]; French, Charette [85], Ploch [380, pp. 91f.]); English, Chomsky & Halle [97]; Yawelmani, Kuroda [280], Ploch [383].

[25]Rennison [406, 407] proposes the notion of tier division to account for the cross-linguistic implications accounted for by GC1a and GC1b.

[26]'Heads must be licensors' ("Heads license operators", Cobb [110, p. 64]) or 'Heads must not be licensors' ("Heads license no operators" (Cobb, [*ibid.*])) are some of the other theoretically possible PE constraints. *At present*, there is however little or no evidence for their proposal (*cf.* Cobb [*ibid.*]).

where the GC 'PEs must be headed' is employed can be found in my analysis of Yawelmani U-harmony (*cf.* Ploch [383]). 'Operators must be licensed', proposed by CG [86, 87] and Ploch [*ibid.*] for a number of Turkic languages and by Cobb [110] for Zulu and Pulaar [pp. 129, 155], eliminates all PEs with unlicensed operators; in other words, in such PEs, the presence of an operator implies the presence of a head. What would follow from this—without any further stipulations—is that 'Operators must be licensed' and 'PEs must be headed' would have the same generative effect; in other words, without theoretical ammendments, only one of these two GCs is necessary.

In addition, there are H/O constraints and LCs on elements. For both types, it is common practice in this version of GP not to refer to more than one element per constraint. The only 'exception' is the I/U fusion parameter which is, as pointed out above, somewhat *ad hoc*. However, this restriction on I/U-fusion may well not be due to a licensing constraint. An H/O constraint on an element restricts an element to a a specific H/O role (head or operator). However, since I have not seen any evidence for either 'Element X must be operator' or 'Element X must not be operator', I will not propose them here. Examples of 'Element X must be head' can be found in CG [86, 87] and Ploch [383] for Turkic languages and Yawelmani (*cf.* below), Walker, on the other hand, proposes 'A must not be head' [485, pp. 109f.] for Vata, and similarly, Cobb has "A cannot be a head" [110, p. 164] for Natal, a variety of Portuguese, and a series of ATR-harmony languages ([110]).

Furthermore, a LC (in my more narrow definition of the term) on an element constrains the licensing properties of an element within a PE. I know at present of no evidence for 'Element X must be licensed'. 'Element X must not be licensed' can be found in English for I ("Nothing can license I", Kaye [262, p. 216]), in Yawelmani for A ("A must not be licensed", Ploch [383]) and in French for U ("Nothing can license U", Kaye [262, p. 217]). 'Element X must be a licensor' is, to my knowledge unattested. 'Element X must not be a licensor', has been proposed for A in an analysis of a number of Turkic languages ("I does not license operators", CG [86, p. 38]; "A is not a licenser", CG [87]),[27] and for the I-element in Turkish and Sakha (Ploch, [*ibid.*]).

Evidently, the theory of GCs is still under construction. However, if we employ only 5 (to 6) elements in combination with 2 fusion (default) parameters and about further 5 GCs, it is possible to predict attested vowel systems and universal implications more accurately than any other theory I know of is able to.

[27] I argue against CG's analyses in Ploch [383].

An important point to mention here is that I do not agree with Cyran's [123] view that phonological derivation may not establish new governing (*i.e.* licensing) relationships or with Backley & Takahashi's [23] assumption and definition of 'SP':[28]

> "Lexical head-complement relations must be retained throughout derivation" (Backley & Takahashi [23, p. 499]).

Similarly, Harris [201] provides an analysis of Chicheŵa height harmony as A-spreading which only works if

> "we make the reasonable assumption that, in accordance with Structure Preservation, lexically established dependency relations remain stable under spreading (Harris [201, p. 535]).

As I have shown in Ploch [383], LCs (in my terminology), *i.e.* constraints on the licensing relationships holding within PEs or those specific to a certain element, may be violated or disregarded at PL while H/O constraints may not.[29] Consider the following data from Sakha and Yawelmani (containing all possible source vowels):[30]

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[28]Backley's [22] version of the ToE does not recognise head- or operatorship. The point relevant here is that Backley & Takahashis's definition of Structure Preservation cannot be maintained in a theory recognising the relevance of H/O roles. For arguments against Backley's approach to Element Theory, *i.e.* tier geometry, *cf.* Ploch [383].

[29]Earlier (in 1994), CG [86] proposed that the fact that U-spreading is more restricted than I-spreading can be explained by assuming the LCs 'U must be head' (a H/O constraint in my terminology) and 'I does not license operators' (also an LC in my account). So U switching its lexically assigned H/O role when spreading is ruled out while I-switching is not due to the different natures of the two 'LCs' involved. However, in their revised account, CG have 'A is not a licenser' instead of 'I is not a licenser' ('I does not license operators'). Since this constraint on A precludes and therefore explains the non-occurrence of A-spreading in CG's view, it is, as I have pointed out in [381] implicit in this later account that LCs (my term), *i.e.* the constraint type which was violable in CG [86], may not be violated at PL; thus the distinction between LCs and H/O constraints referred to in [86] was abandoned in [87].

[30]Regarding Sakha, *cf.* CG [86, 87] and fieldwork notes by Charette; regarding Yawelmani, *cf.* Kuroda [280] and Kaye [259]. Also, I neglect 'long' vowels in Yawelmani here. For a discussion of such vowels and their vowel harmonic behaviour, *cf.* Ploch [383]. There I claim that there is a constraint in Yawelmani (at PL) that PEs which are attached to (the head of) a branching nucleus acquire an A-operator at PL; this constraint leaves PEs which already contain A at LL unaffected.

(10) *Sakha and Yawelmani U-harmony (data)*

	Sakha				*Yawelmani*			
	Nom.	*Def.acc.*	*Plural*	*Gloss*	*Stem*	*Non-fut.*	*Dub.*	*Gloss*
ɨ	ɨt	-ɨ	-tar	'dog'				
a	at	-ɨ	-tar	'horse'	xat-	-hin	-al	'to eat'
u	bultʃut	-u	-tar	'hunter'	dub-	-hun	-al	'to lead by hand'
o	oɣo	-nu	-lor	'child'	bokʔ-	-hin	-ol	'to find'
i	kiːs	kiːh-i	-ter	'sable'	xil-	-hin	-al	'to tangle'
e	mektʃirge sijemex	-ni	 -ter	'owl' 'carnivor'				
y	yt	-y	-ter	'milk'				
ø	børø	-ny	-lør	'wolf'				

Based on my analysis presented in Ploch [*ibid.*], I generate the lexical nuclear PEs of Sakha and Yawelmani with the following GCs:[31]

(11) *GCs in Sakha and Yawelmani*

	Sakha	*Yawelmani*
1.	Operators must be licensed.	PEs must be headed.
2.	A must be head.	I must be head.
3.	I must not be a licensor.	A must not be licensed.

A comparison of Sakha *versus* Yawelmani in relation to GC 1 in (11) demonstrates the differentiation of 'Operators must be licensed' (LC) and 'PEs must be headed' (H/O constraint).

In addition, (12) shows the derived PEs generated by a rightward spreading process of U at the nuclear projection, assuming, and this is the crux of my analysis, that the spreading element, here U, may well change its dependency status but may not switch its lexically assigned H/O role ("non-switch harmony", *cf.* Ploch [383]):[32]

[31]I assume that the GC "Elements may fuse"—which I do not discuss in Ploch [383]—is ON in both Sakha and Yawelmani, while "I and U may fuse" is ON in Sakha but OFF in Yawelmani.

[32]Note that in Sakha, but not in Yawelmani, there is also unrestricted rightward I-harmony at the nuclear projection. Furthermore, the phonetic realisation of an unlicensed, *i.e.* audible, empty nucleus is [ɨ ~ ɯ] in Turkic languages (*cf.* CG [*ibid.*]) but [i] in Yawelmani (*cf.* Kaye [259]).

(12) *Sakha and Yawelmani U-harmony (PEs)*

Sakha Stem-V (LL)	*Def.acc.* (PL)	*Plural* (PL)	*Yawelmani* Stem-V (LL)	*Non-fut.* (PL)	*Dub.* (PL)
ɨ	ɨ	a ($\underline{A}$)			
a ($\underline{A}$)	ɨ	a ($\underline{A}$)	a ($\underline{A}$)	i	a ($\underline{A}$)
u ($\underline{U}$)	u ($\underline{U}$)	a ($\underline{A}$)	u ($\underline{U}$)	u ($\underline{U}$)	a ($\underline{A}$)
o (U · $\underline{A}$)	u (U)	o (U · $\underline{A}$)	o (U · $\underline{A}$)	i	o (U · $\underline{A}$)
i ($\underline{I}$)	i ($\underline{I}$)	e (I · $\underline{A}$)	i ($\underline{I}$)	i	a ($\underline{A}$)
e (I · $\underline{A}$)	i (I)	e (I · $\underline{A}$)			
y (I · $\underline{U}$)	y (I · $\underline{U}$)	e (I · $\underline{A}$)			
ø (I · U · $\underline{A}$)	y (I · U)	ø (I · U · $\underline{A}$)			

In this analysis, I assume the unlicensed (audible) nucleus of the Sakha definite accusative and the Yawelmani non-future suffix to be lexically empty and the one of the Sakha plural and the Yawelmani dubitative suffix to dominate lexical ($\underline{A}$). If we now define U-harmony in either language as rightward U-spreading at P_N restricted by a non-switch constraint, we predict the patterns in (12) as long as we assume that the GC 'Operators must be licensed' (Sakha) may be disregarded at PL while 'PEs must be headed' (Yawelmani) may not. This distinction between LCs and H/O constraints enables us to explain why an U-operator may spread from Sakha o (U · $\underline{A}$) into an empty nucleus, thus creating u (U) at PL, a PE which is ungrammatical at LL, while it may not do so in Yawelmani.[33]

It seems that a view which differentiates LCs from H/O constraints in relation to SP is preferable to accounts which outlaw all derived changes of lexically established

[33]Based on data provided by Doerfer, Hesche & Scheinhardt [139], Li [295] and Malchukov [314], Toft [476] analyses Even (also called 'Lamut'). Even though she does not discuss this, the explanation with the fewest stipulations (of a set of nine competing explanations provided by her) of the attested palatal harmony assumes that all PEs are headless at LL and PL and that there is thus no switching of spreading elements or target heads; there is no U-harmony in Even. Since in a system with no headed vowels none of the PE constraints or H/O constraints or LC constraints play any role, the only GC necessary to predict the Even facts are: 'I and U may fuse' (presuming 'Elements may fuse'), resulting in the lexical (and phonological) PEs: i (I), u (U), ɑ (A), e (A · I), o (A · U), y (I · U), ø (A · I · U) plus ɨ, the realisation of an unlicensed empty nucleus. I spreads from all vowels containing I, *i.e.* from i e y ø, into all possible target PEs, *i.e.* into (A), (U) and the empty nucleus, resulting in three types of recessive (non-leftmost) nuclei: a (A) alternating with e (A · I), u (U) alternating with y (I · U), and ɨ (unlicensed empty nucleus) alternating with i (I). Since the acquirer needs no help in deciding which of the PE, HO and licensing constraints are relevant (because none of them are in Even), non-switch U-harmony—which fulfills this purpose in Turkish and Sakha—is not necessary and does therefore not occur in Even.

dependency relationships (as proposed by Harris, Cyran and Backley & Takahashi). Finally, note that according to Backley & Takahashi's definition of SP, lexical head-complement relations must be retained throughout derivation irrespective of any further specifications. This means that *all* lexical licensing relations—be they melodic, *i.e.* PE-internal, or syllabic, *i.e.* between skeletal points or constituents—must be preserved at PL in their approach. I agree with a syllabic version of such a constraint. Syllabic SP does however in no way imply melodic SP. It is not even clear or 'established' that the relationship holding between the head and an operator within a PE—usually referred to as 'licensing'—is linked to, connected with or in any phonologically relevant way identical or similar to the 'licensing' relationship holding at the skeleton between the head and its complement (*cf.* KLV [267]) or between a nucleus and the onset to its left which it 'licenses' (*cf.* Kaye [257]), KLV [267, pp. 200f.] and Vergnaud [483].[34] These three types of binary asymmetrical relations might as well be unconnected. Simply assigning one and the same label 'licensing' to all three types does not establish any such link.[35]

To sum up, in this section I have looked at the basic concepts that are employed within GP to predict melodic patterns in phonology. This involved an introduction to the Theory of Elements and the Theory of Generative Constraints and a discussion of the reasons why I reject Cyran's [123] proposal that phonological derivation cannot create new government relationships and, similarly, Backley & Takahashi's [23] definition of SP which bars *any* lexical head-complement relations from being altered at PL.

[34]According to Kaye, "each nucleus can and must license an onset to its left" (Kaye [257]). This is intended to exclude onsets which are not licensed by a nucleus to their right, *e.g.* onset_1 in a onset_1–onset_2–nucleus sequence, but, in my opinion, fails to do so. Similarly, Harris's [199, p. 380] 'Onset Licensing Principle' stipulates that "an onset head position must be licensed by a nuclear position". Since not all onset positions are heads and since only some onsets dominate a skeletal point (defining a position), this principle too fails to predict what is has been assumed for: that each onset must be licensed by a nucleus to its right and, *simultaneously*, that each nucleus must license an onset to its left, or, in other words, that onsets and nuclei always come in pairs. Even if Harris's definition included all onset types, there could still be nuclei which do not license an onset.

[35]Let me add that there *are* cases where the phonology links melodic to syllabic licensing. For example, universally, PEs dominated by the *head* of a branching nucleus (which thus licenses a nuclear complement to its right at the skeleton) must be (melodically, *i.e.* PE-internally) *headed*. This, however, has been established independently (*cf.* Kaye [258]); there is no *a priori* motivation for this purely terminological link.

4.2 A 'nasal' element

In this section, I would like to introduce the nasal element N which was used in GP at least until 1993 to motivate phonetic nasality and update the findings made during this period by showing in what way such research supports my view that there can be no empirical discussion of phonological nasality based on the PH and that nasality is the phonetic realisation of a cognitive concept. This will provide the background for my discussion of the merger of N and L in chapter 5.

Since KLV [265, p. 311], GP has assumed a nasal element, *i.e.* the N-element, as part of its comparatively small set of such elements. In KLV [265, p. 311], *i.e.* before the abandonment of charm theory, N was positively charmed (N^+). One of the arguments against charm theory is that, by declaring fusion of identically charmed elements ungrammatical, it disallows fusion of A^+ and N^+ and thus predicts incorrectly that, universally, NVs may not contain A. As will become evident below, this is simply wrong; on the contrary, a cross-linguistically recurrent (but in no way universal) pattern in relation to NVs is that they must contain A. Another problem was that it had to be stipulted that, like the 'ATR'-element, *i.e.* like $ɨ^+$, N^+could only occur as operator, never as head (*cf.* Tourville [477, p. 8]).

One of the first discoveries I made when I started to investigate nasality was that only little cognitive research had been done. Since KLV's [*ibid.*] article, nasality which seemed to be phonologically relevant, *e.g.* in nasal stops (as universally obligatory natural class) or as 'active ingredient' involved in nasal harmony phenomena, was motivated by the presence of an N-element (*cf.* Bamba & Nikiema [27], Brockhaus [66, p. 196], [67], Harris [198, p. 264], Heo [211, pp. 30ff], Rhee [408, p. 129], Tourville [477]) or was left to future research:

> "Nor will we have anything to say about nasality; given the current state of our knowledge, it is not clear whether this should be represented by an autonomous nasal element or is more appropriately subsumed under one of the laryngeal elements. (Harris & Lindsey [205, p. 65])."

Note that I am not trying to criticise Harris & Lindsey's article for not discussing the motivation of the nasal element. I simply want to point out that, since the mid of 1980s, the nasal element, though commonly employed, is rarely (if ever) the topic of cognitive research.

Interestingly, in the above quote from Harris & Lindsey, the authors speculate whether phonological 'nasality' might be captured by "one of the laryngeal elements" [*ibid.*], *i.e.* (presumably) by L or H. To my knowledge, the only element which has been proposed as possible candidate for this purpose is the low tone element, *i.e.* L (*cf.* Kaye [258]). I will look at this proposal in more detail in chapter 5.

In this chapter, however, I would like to present the main findings of Bamba & Nikiema's ('BN') report from 1986 on the phonological representation of nasality from [27] and Tourville's study of nasality in Jula from the same year [477]. Both of these papers are relevant to our discussion of nasality because they are the two main GP-based papers on nasality. Note however that BN and Tourville still assume charm theory which I will only represent where necessary.

BN's objectives are two-fold: firstly, to establish a phonologically relevant contrast between nasal vowels and nasal diphthongs—'diphthongs' as argued for by Kaye [252]—and, secondly, to come up with a formal approach to nasal segments [p. 1]. As starting point of their investigation, the authors refer to the work of Leben [291], Goldsmith [178], Vergnaud [483], Bécuwe [33] and "Hyman (1982)"[36] who established the assumption of an autosegment corresponding to nasality. The data BN employ for their argumentation comes from two languages: Mahu ("mahou", Bamba [26]), a Manding language (Ivory Cost), and Lobiri (Bécuwe [33]), a Gur language spoken in Burkina Faso.

To illustrate BN's proposal, consider the following facts about Mahu. In Mahu, there are seven oral and seven corresponding nasal vowels: i u a e o ɛ ɔ, and ĩ ũ ã ẽ õ ɛ̃ ɔ̃. Interestingly, the lax low NVs sometimes (but not always) nasalise a right-adjacent segment:[37]

(13) *Two types of non-high lax NVs in Mahu; example:* ɔ̃ + -la

a.	sɔ̃$_1$	la	→	sɔ̃$_1$na
	gazelle	opening		'mouth of a gazelle'
b.	sɔ̃$_2$	la	→	sɔ̃$_2$la
	leaf	opening		'door made of palm leaves'

Evidently, in (13a.), ɔ̃$_1$ triggers nasalisation of the following consonant—turning l into n—, while ɔ̃$_2$ does not—l remains unchanged. So let us define type 1 ('t1') NVs as NVs which *do* trigger nasalisation and type 2 ('t2') NVs as those which do *not*. In a revised version of Element Theory (as presented in section 4.1.3), we could

[36] There is no such entry in BN's bibliography. BN probably refer to Hyman [235].

[37] Since BN do not distinguish ɔ̃$_1$ from ɔ̃$_2$ orthographically, it is implicit in their presentation that the two NVs that I have labelled 'ɔ̃$_1$' and 'ɔ̃$_2$' sound identical.

analyse the nasalisation phenomenon triggered by t1 NVs as strictly local (binary) L-spreading. Neglecting the head/operator role L has in a t1 NV, we can say that L spreads into l (A·ʔ), resulting in n (L·A·ʔ)—more about consonants in section 4.1.3. Let me add that the same difference in behaviour between $\tilde{ɔ}_1$ and $\tilde{ɔ}_2$ can also be observed when adding the vowel-initial (*i.e.* empty onset-initial) suffix -o to the two stems given in (13):

(14) *Two types of non-high lax NVs in Mahu; example:* ɔ̃ + -o

a.	$s\tilde{ɔ}_1$	o	→	$s\tilde{ɔ}_1ŋo$
	gazelle	*definite*		'the gazelle'
b.	$s\tilde{ɔ}_2$	o	→	$s\tilde{ɔ}_2o$
	leaf	*definite*		'the door'

Apparently, when the suffix-initial onset is empty, N in t1 NVs (but not t2 NVs) spreads to that onset, resulting in ŋ intervening between t1 NV and the suffix vowel.

BN point out that, to explain these two types of NVs, nasality could in an autosegmental framework be assumed to be a suprasegment (which can spread) in $\tilde{ɔ}_1$ but an autosegment (which may not spread) in $\tilde{ɔ}_2$. I agree with Pulleyblank [403] (*cf.* section 2.5.1, pp. 73ff.) who argues—independently of nasal spreading—that the main improvement of an autosegmental view over SPE-type transformational rules is that autosegmental theory overgenerates to a lesser extent and involves a higher degree of non-arbitrariness. However, BN argue that even though an autosegmental approach is able to correctly predict the observations, it cannot explain them or, as far as I understand their argumentation, be backed up by independent evidence.

BN also provide data from Lobiri which exhibits a vocalic system consisting of ten oral (i u a e o a ɩ ʊ ɛ ɔ ɨ)[38] and ten corresponding nasal vowels (ĩ ũ ã ẽ õ ã ɩ̃ ʊ̃ ɛ̃ ɔ̃ ɨ̃). The main differences between Mahu and Lobiri are three-fold: Firstly, while in Mahu there are seven t2 NVs (not triggering nasality spreading) and three (low and lax) t1 NVs (triggering nasality spreading)—next to seven OVs corresponding to the seven t2 NVs, Lobiri displays a system without t2 vowels; in other words, all Lobiri (t1) NVs (not only the low or some of the low NVs as in Mahu) appear to be able to trigger nasalisation. Secondly, in Mahu there are no high t1 NVs, while in Lobiri vocalic nasality (all of which is of t1) is not restricted to non-high vowels. Note also that in Lobiri nasality does not spread but moves, in other words, the nasality on Lobiri (t1) NVs is detached from the vowels and moved to the following segment,

[38]BM have 'ʌ' for 'ɨ'.

the vocalic triggers themselves denasalise. The examples BN give are:[39]

(15) *Nasality movement in Lobiri*

fɩna	fɩna-ra	
your ox	'your ox in question'	
fɩnã	fɩna-na	*fɩnãra
your millstone	'your millstone in question'	
ʔa-dɩhɩ	dɩhɩ-rɛ	
if-this:spoils	'this:spoils-*affirmative*'	
ʔa-dɩhɩ̃	dɩhɩ-nɛ̃	*dɩhɩ̃-rɛ
if-this:carbonises	'this:carbonises-*affirmative*'	
hɩ̃	hɩ-nɛ̃	*hɩ̃-rɛ
to carbonise	'to carbonise-*affirmative*'	

BN do not refer to Tourville [477], but let me sum up the results of Tourville's study relevant here.[40] In Jula, a language ('belonging to' the Mandé group of the Niger-Congo family), there are seven oral vowels (i u a e o ɛ ɔ) and seven corresponding vowels involving nasality which we could represent phonologically, *i.e.* cognitively, as vowels containing an N-element (represented by ĩ ũ ã ẽ õ ɛ̃ ɔ̃). However, i u e o are, in fact, never phonetically nasalised but can, in some cases, be analysed phonologically as PEs containing an N-element. The situation is as follows: In isolation, *e.g.* domain-finally, there are seven oral vowels; such phonologically non-nasal vowels never trigger nasalisation of the first onset of a following morph. Then, there are the three (t2) NVs ɛ̃ ɔ̃ ã which are always audibly nasalised, *i.e.* also domain-finally; ɛ̃ ɔ̃ ã do not trigger nasalisation.[41] Phonologically (and phonetically) oral i u e o, on the other hand, contrast with domain-final (only phonologically but not phonetically nasalised) ĩ ũ ẽ õ (t1). These t1 NVs are perceptually indistinguishable from underlyingly oral i u e o when in isolation and trigger nasalisation of the next onset

[39]There is also a phonemenon (which does not affect pronouns) which spreads nasality from an onset to a following nucleus. If—as the data might imply—this means that nasality moves from a nucleus to a following onset from which it may spread to the following nucleus (if no pronouns are involved), we can say in relation to nasality in Lobiri that, apparently, both strictly local rightward movement and (where applicable) stricly local rightward spreading occur.

[40]Tourville's points to Braconnier [58, 59], Braconnier & Diaby [60], Braconnier, Maire & Tera [61], Dumestre & Retord [145], Dunn & Rouzier [146] and Limoges [296] for further data on Jula.

[41]There are a few problematic cases with suffix-initial l. Dependent on the l-initial suffix, l may never be nasalised or is always nasalised when the preceding vowels is either a t1 or a t2 NV. I will neglect the exceptional behaviour of l in the following.

to their right when followed by another domain. The following list gives examples of all 14 vowels in domain-final position:

(16) *Jula vowels*

a. *Oral vowels*			b. *Nasal vowels*		
bí	[bí]	'to scoop'	bĩ́	[bí]	'herb'
kúlu	[kúrú]	'yesterday'	kúlũ	[kúrú]	'dug-out'
sé	[sé]	'to be able to'	sẽ́	[sé]	'bridge'
sò	[sò]	'horse'	sõ̀	[sò]	'thief'
g͡bɛ̀lɛ	[g͡bɛ̀rɛ̀]	'to approach'	g͡bɛ̀lɛ̃	[g͡bɛ̀rɛ̃̀]	'to cease'
sɔ́	[sɔ́]	'village'	sɔ̃́	[sɔ̃́]	'antilope'
sà	[sà]	'to die'	sã̀	[sã̀]	'to buy'

The same asymmetry between t1 and t2 NVs which can be observed in (16) with respect to the (non-)realisation of vocalic nasality in isolation can also be made evident by looking at cases where NVs are followed by another morph. Consider the following data (again taken from Tourville [477]):[42]

(17) *Type 1 and type 2 nasal vowels in Jula*

a. Nasality movement

fĩ́-ja	fíɲá
black-*nominal*	'blackness'
sùlũ-ja	sùrùɲà
small-*nominal*	'smallness'
fwẽ́ dẽ́-nĩ	fwé^{n}dèní
blind child-*diminutive*	'blind baby'
bõ̀-ja	bòɲà
thick-*nominal*	'thickness'

[42]Note that the vowel in the diminutive suffix -nĩ in (17) is realised without nasality. This means that Tourville probably has data which shows that the final vowel of the diminutive suffix triggers nasalisation of the first onset of a following domain. Also, I use 'j' for Tourville's 'y'.

b. No nasality movement

fyɛ̃́-ja	fyɛ̃́-já
easy-*nominal*	'easiness'
g͡bɔ̃̀ dẽ́-nĩ́	g͡bɔ̃̀dènì
baboon child-*diminutive*	'baboon infant'
kùnã-ja	kùnã̀jà
bitter-*nominal*	'(the state of) being bitter'

As the data in (17) illustrate, the NVs of Jula can be subcategorised into two sets: type 1 NVs (ĩ ũ ẽ õ) trigger rightward nasality movement, type 2 NVs (ɛ̃ ɔ̃ ã) do not. Note that while in Lobiri all NVs—high, mid and low ones—are of type 1 (triggering nasality movement) and that while in Mahu all type 1 NVs are lax and mid/low, in Jula it is the lax low NVs which are type 2 and the high and tense mid vowels which are of type 1, *i.e.* are involved in nasality movement. Furthermore, in Mahu and Lobiri, vocalic nasality is audible independently of whether t1 or t2 NVs are involved and, in the case of a t1 NV, nasality is audible on the vowel independently of whether it can move/spread to the right; in Jula, on the other hand, t1 nasality is only audible if it can spread, otherwise it is delinked from the nucleus it is lexically attached to.[43]

If we continue to define t1 NVs as those which spread or move the nasality associated with them and t2 NVs as the ones not taking part in such phenomena, we can provide a synopsis of the above via table 4.1 (p. 183).

BN's explanation of the difference in behaviour between t1 and t2 NVs makes use

[43]Alternatively, we could say that N in t1 vowels is a floating element which, in Jula, can never be linked to the nucleus in the phonological proximity of which it is generated lexically but must be associated with an onset to its right; otherwise N remains unconnected to the skeleton and thus inaudible. This, of course, opens up a whole new research topic: floating elements. Interesting here would be to gain knowlege regarding how an element floats or what the restrictions on floating are. So can there only be domain-final floaters, domain-initial ones, or can each skeletal point not only be associated with melodic material it dominates but also with a number of floating elements? If yes, how does a floating element know around which skeletal point it is floating, considering that it is, as floater, unattached? It seems to me that domain-internal floaters presuppose a 'ghost skeleton', *i.e.* a copy of the skeleton which is in sync with the skeleton and which floaters are linked to. Basically, only melodic material linked to the real world skeleton would be pronounced in this view. I can unfortunately not discuss this in more detail here. However, if the assumption of domain-internal floaters is necessary, it would be interesting to see whether this would provide credence for Backley & Takahashi's [23] *activate* α approach, in which all universally possible elements are always present at any skeletal position but where they need to be (licensed to be) activated in order to be phonetically realised. For more information on floating consonants, *cf.* KLV [265, p. 324] (who still propose a tier for each element), Prunet [401, p. 227] (*cf.* (5) in chapter 6, p. 234) on French NVs, and Kaye [253, pp. 118ff.] on floating tones in Dida.

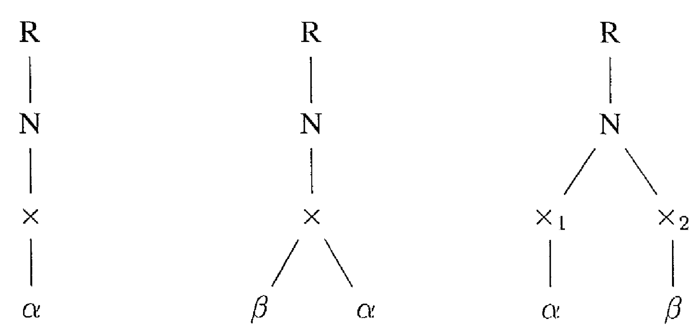
Table 4.1: Vowels in Mahu, Lobiri and Jula (summary)

a. *Mahu*			b. *Lobiri*			c. *Jula*		
OVs	*t1 NVs*	*t2 NVs*	*OV*	*t1 NVs*	*t2 NVs*	*OVs*	*t1 NVs*	*t2 NVs*
i		ĩ	i	ĩ		i	ĩ	
u		ũ	u	ũ		u	ũ	
			ɩ	ɩ̃				
			ʊ	ʊ̃				
e		ẽ	e	ẽ		e	ẽ	
o		õ	o	õ		o	õ	
ɛ	ɛ̃	ɛ̃	ɛ	ɛ̃		ɛ		ɛ̃
ɔ	ɔ̃	ɔ̃	ɔ	ɔ̃		ɔ		ɔ̃
a	ã	ã	a	ã		a		ã
			ɨ	ɨ̃				

of Kaye's [252] distinction of light from heavy diphthongs—which Kaye established by looking at a number of West African languages. Let me therefore first introduce the relevant theoretical concepts in relation to diphthongs. (18a.) exhibits the structure of a non-branching nucleus dominating one PE which contains all the elements associated with that nucleus; all elements are fused. In (18b.), on the other hand, we see a light diphthong, (18b.), *i.e.* a structure in which all elements are dominated by one nucleus but where not all elements linked to that one nuclear point are fused. The representation of a heavy diphthong is given in (18c.); as in (18b.), the elements associated with the nucleus are linked to that nucleus in two groups. However, in (18c.), these two groups are dominated by two separate points which belong to one and the same branching nucleus. In line with KLV [267], the left-hand position ($\times_1$) licenses the right-hand position ($\times_2$) via constituent government.

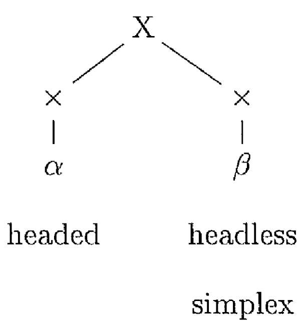
(18) *Light* vs. *heavy diphthongs*

a. *Monophthong* b. *Light diphthong* c. *Heavy diphthong*



β may only be filled by sonorants (semi-vowels, liquids and nasals).[44] Note that there is no phonologically relevant ordering between α and β in a light diphthong (18a.); α and β simply symbolise two PEs which are not fused and between which there is no government or any other kind of licensing relationship. While BN try to predict the substantive constraints on α and β in a heavy diphthong via charm theory, let me add here that in a revised version of the ToE—*i.e.* in line with Kaye [258]—these substantive constraints on constituent governors and governees can be summed up as in (19):[45]

(19) *Substantive constraints on constituent government*



We could, for example, substitute α for a ($\underline{\mathrm{A}}$) and β for ι (I) or ʊ (U), resulting in the heavy diphthong aι or aʊ, *e.g.* in English ⟨guy⟩ or ⟨cow⟩. For nuclei, we have to add that it appears to be a common restriction that α has to while β may not contain an A-element—outlawing *e.g.* uι, iʊ, ιa, ʊa, ιɔ or ʊɛ. Additionally, for onsets and nuclei it can be observed that an element may not be associated with both governor and governee of a branching constituent; for example, this makes pw (with both p and w containing an U-element) ill-formed as branching onset in English, but leaves tw well-formed; similarly, aι ($\underline{\mathrm{A}}$)(I) is grammatical, ɛι (I · $\underline{\mathrm{A}}$)(I) is not.[46]

Note that, even though theoretically there is no sequential ordering between α and β in a *light* diphthong, Kaye [258] assumes that *e.g.* aι or aʊ always corresponds to a heavy diphthong (with this ordering predicted by the formal and substantive constraints on constituent government), while a light diphthong will obligatorily display the reverse order, *e.g.* in French wa in ⟨oie⟩ wa 'goose'.[47] For the purposes of our

[44]These phonetic terms are merely descriptive and are only useful when used in this overgeneralising manner.

[45]'X' in (19) stands for 'nucleus' or 'onset'; rhymes appear not to be subject to the same constraints. Unfortunately, I cannot discuss this distinction any further here.

[46]As Kaye [262] has shown, English ɛι—as in ⟨say⟩—does not consist of two separate PEs each of which is linked to two separate points belonging to the same nucleus (heavy diphthong) but is made up of only one PE (A · $\underline{\mathrm{I}}$) dominated by two nuclear points.

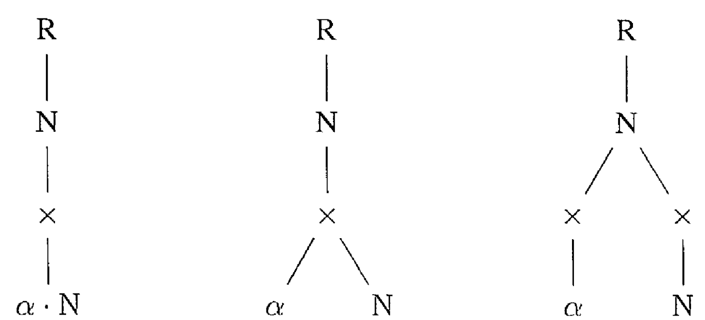
[47]That w in ⟨oie⟩ is not dominated by a word-initial onset but that this onset must be empty and

discussion here, it suffices to say that heavy diphthongs pattern with other branching nuclei ('long vowels'), *e.g.* they may not be followed by coda-onset sequences (neglecting coronal coda-onset sequences), while light diphthongs pattern with non-branching nuclei. Also, it seems that there *are* examples where aɩ or aʊ behave like light diphthongs. For example, in Yimas, a language spoken in New Guinea, there are according to Foley [167, pp. 44ff.] the four short vowels a i u ɨ—*i.e.* (A) (I̲) (U̲)[48] and the phonetic realisation of an unlicensed empty nucleus—and the two diphthongs aɩ and aʊ which have "a strong tendency to be ... realized phonetically as /ey/ and /ow/" [167, p. 46]; aʊ is even commonly monophthongised to ɔ. In my opinion, this simply means that Yimas exhibits the common five vowels system a i u e o with the elements contained in e and o (*i.e.* A and I, and A and U, respectively) fused but realised phonetically in sequence, *i.e.* not even light diphthongs are involved here.

Let me now introduce BN's distinction of NVs and two types of nasal diphthongs (*cf.* BN [27, pp. 8f., 11f.]). A NV (in a narrow sense, *i.e.* not a nasal diphthong) consists of a PE which contains an N-element fused with other elements; this one PE is linked to one (non-branching) nucleus (20a.). In addition to that, BN propose that, universally, there are two types of nasal diphthongs, those involving a light diphthong with 'oral' and 'nasal' melodic material separately linked to one and the same nuclear point (20b.) and those motivated by a heavy diphthong, where nasality (not fused with any other elements and audible as "la *nasale pure*" [p. 8]) is linked to the recessive (right-hand) point of a branching nucleus:

(20) *BN's three types of nasal nuclei*

a. *Monophthong* b. *Light diphthong* c. *Heavy diphthong*



not dominate a point can be seen in the definite form ⟨l'oie⟩ lwa (*⟨la oie⟩ *lawa). If the onset did dominate a point, the conditions for the disappearance of a in la̲ would not be met, as they are not in ⟨ouate⟩ wat 'cotton' with ⟨la ouate⟩ lawat (*⟨l'ouate⟩ l'wat)—for those speakers which make this distinction.

[48] I will discuss differences in cross-linguistic patterns of head/operator assignment for A, I and U in more detail in chapter 6.

Neither BN for their analysis of Mahu and Lobiri NVs nor Tourville for her account of Jula NVs use heavy nasal diphthongs. However, since my account of vocalic nasality in chapter 6 will, amongst other proposals, refer to heavy NDs, let me start the discussion of BN's nasal nuclei by pointing to the main advantage of a heavy ND account. A heavy nasal diphthong ('ND') as in (20c.) is in BN view pronounced [αŋ]; so, I presume, [aŋ] would be the phonetic realisation of a heavy nasal diphthong with [a] filling in for α; for BN, the PE motivating [a] would be ($\underline{A}^{\circ}$), for me, ($\underline{A}$). The question which arises here is: do heavy NDs actually occur in any language, or, how would we know of them? For example, if there were any examples where a language-specific restriction holding for branching oral vowels but not for non-branching oral vowels would also hold for short vowels preceding ŋ but not for other short vowels, we would possibly have found evidence in favour of the proposal of heavy NDs. We could say that all branching nuclei in this kind of language have to dominate a PE containing A.—Such an example exists. As I will show in chapter 6, in the French spoken in the area around Montpelier ('MF'), there are no NVs; however, domain-final Parisian French or Québec French ('QF') NVs correspond to sequences 'oral vowel plus ŋ' sequences in MF. Parisian or QF NVs which are followed by a consonant show in MF oral vowels followed by a nasal stop homorganic to the onset it precedes. Let me illustrate this point with data from QF and MF:[49]

(21) *Nasal vowels in Québec and Montpelier French*

			QF	*MF*	*Gloss*
a.	ã ~ aN	⟨jambe⟩	ʒãb	ʒambə	'leg'
		⟨banquet⟩	bãkɛ	baŋke	'banquet'
		⟨vent⟩	vã	vaŋ	'wind'
b.	õ ~ ɔN	⟨bombe⟩	bõb	bɔmbə	'bomb'
		⟨mondial(e)⟩	mõdjal	mɔndjal	'word-wide'
		⟨bâton⟩	batõ	batɔŋ	'stick'
c.	ø̃ ~ œN	⟨humble⟩	ø̃b(l)	œmblə	'humble'
		⟨humblement⟩	ø̃bləmã	œmbləmaŋ	'with humility'
		⟨brun⟩	brø̃	brœŋ	'brown (m)'

[49]Data mine.

d.	ẽ ~ εN	⟨simple⟩	sẽp(l)	sεmplə	'simple'
		⟨cinquième⟩	sẽkjεm	sεŋkjεmə	'fifth'
		⟨vingt⟩	vẽ	vεŋ	'twenty'

Interestingly, all MF sequences of 'oral vowels plus ŋ' exhibit a ε ɔ œ but never i u y; so such vowels must be non-high. Also, the contrast between lax and tense mid vowels is neutralised. We find the same constraints in relation to QF NVs: they too are obligatorily non-high, and there is no opposition between lax and tense mid vowels. Note that elsewhere QF distinguishes high from mid from low vowels and exhibits tense-lax contrasts (*cf.* chapter 6). One could claim that, therefore, MF vowel-ŋ sequences are branching nuclei, and that MF branching nuclei are subject to a must-contain-A constraint which cannot only also be found in QF but also in Yawelmani (*cf.* Ploch [383]).

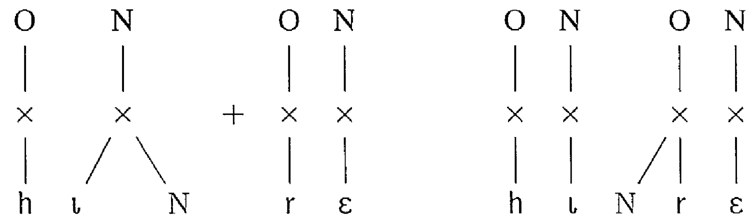
Unfortunately, there is no independent evidence for branching nuclei in MF. That is to say that the only vowels subject to the must-contain-A constraint in MF are those preceding ŋ (or preceding a nasal stop which is homorganic to the following consonant). We therefore have to propose branching nuclei for MF and add the language-specific stipulation that β, *i.e.* the recessive position of branching nuclei, may only dominate an N-element. As I will discuss in more detail in chapter 6, there is cross-linguistic evidence for such a restriction—even though BN do not provide any further evidence. For now it suffices to say that I will explain in chapter 6 why some languages which have no branching nuclei dominating N-less melodic material may employ branching nuclei for NVs.

Let us now look at how BN employ nasal monophthongs ('NMs') and light NDs to explain the difference between t1 and t2 NVs. BN's main idea here is that t2 NVs, *i.e.* those *not* triggering spreading or movement of nasality, correspond to NMs, while t1 NVs (triggering nasalisation effects) are motivated by light NDs; t1 NVs—movement/spreading—NDs, t2 NVs—no effect—NMs. Basically, the possibly intuitively pleasant assumption is that an N-element in a light ND, which is not fused with the oral part of the vowel, is less permanently attached to it than an N-element which is fused with it. This results in the following analysis of Lobiri NVs as light NDs, as illustrated by ĩ in hɩnε (15), a t1 NV (like all Lobiri NVs) moving to the following onset dominating r and turning r into n (*cf.* BN [27, p. 11]):[50]

[50]In the version of GP I employ, all nuclei—be they branching or non-branching—are dominated by a constituent 'rhyme' which is not included in the following representations.

(22) *Lobiri nasality movement from t1 NVs (according to BN)*

a. hĩ + -rɛ   b. hɩnɛ

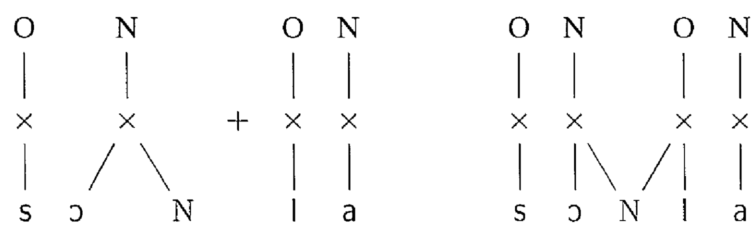


Let me point out that it is not necessary to assume that hɩnɛ is phonologically derived from hĩrɛ, *i.e.* that n is derived from r. The concatenation of hĩ and rɛ might as well be lexical—but this is not relevant here. Note that I will look at the internal representations of the consonants involved in nasalisation phenomena in chapter 7.[51]

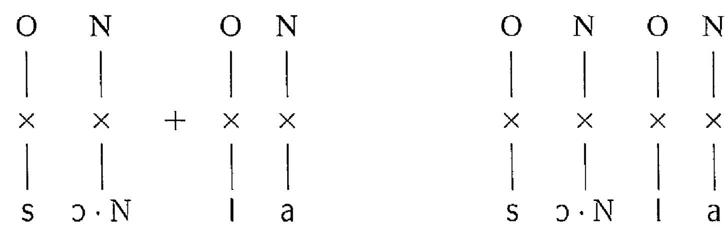
For Mahu, BN assume two types of NVs, *i.e.* light NDs for the three Mahu t1 NVs and seven NMs motivating the seven t2 NVs attested (*cf.* table 4.1, p. 183). BN's analysis of the Mahu examples in (13) is as follows:

(23) *Nasality spreading in Mahu (according to BN)*

a. sɔ̃$_1$ + -la   b. sɔ̃$_1$na



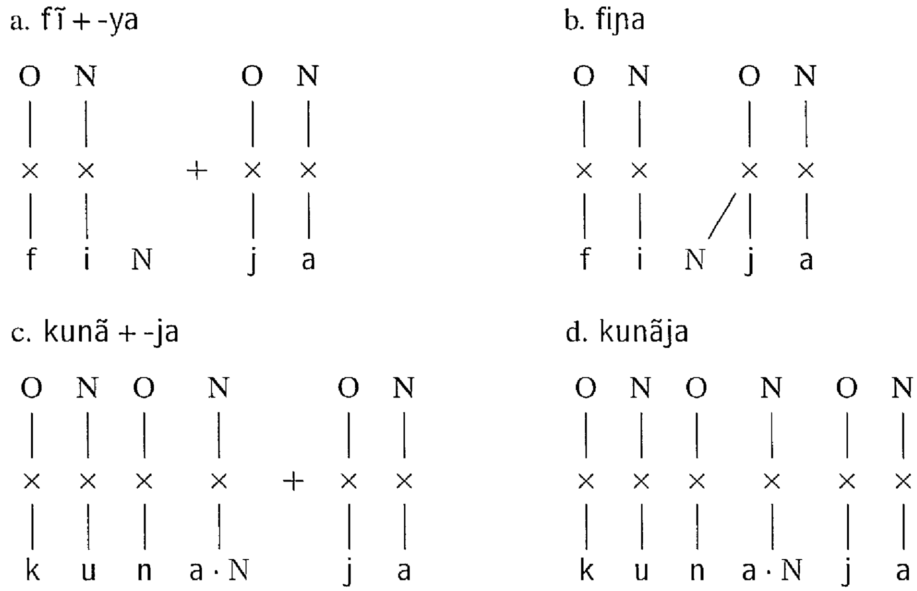
c. sɔ̃$_2$ + -la   d. sɔ̃$_2$la



Tourville does not discuss in detail the differences between Mahu or Lobiri t1 NVs, on the one hand, and Jula t1 NVs, on the other. I would like to suggest that, given BN's distinction of light NDs from NMs, the apparently deviant behaviour of Jula t1 NVs can be explained by a distincion of light NDs for Mahu and Lobiri (as in (23a.–b.)) from floating N for Jula (24a.–b.):

[51]It is sufficient here to add that in the revised ToE as presented in section 4.1.3 and in chapter 7, r (A) plus N (ʔ · N) results in n (ʔ · N · A).

(24) *Association of nasality in Jula*



Note that in Jula, the nasality on t1 NVs, *i.e.* floating N, can only be linked to the leftmost onset of the morph which follows the domain N is associated with lexically. This is why the stem observable in (24a.–b.), *i.e.* fĩ, is, when in isolation, not pronounced *[fĩ] but [fi]. NVs of t2, on the other hand, neither move nor spread nor float; they are NMs, with N fused with the rest of the melodic make-up of the vowel it nasalises.

To sum up, in this section I have looked at BN's paper on nasality in Mahu and Lobiri. Based on their findings—and after having added the distinction of t1 NVs motivated by floating N *versus* t1 NVs motivated by a light ND—I can now claim that there is evidence for four types of nasal vowels: Firstly, NMs (t2 NVs in Jula, Mahu, Lobiri), secondly, heavy NDs (*e.g.* in QF or MF), thirdly, light NDs (triggering nasal spreading or movement) and, fourthly, vowels with floating N (for t1 NVs in Jula). The distinction between light and heavy NDs was proposed to account for the presence of the must-contain-A constraint—for which there is only evidence for branching nuclei—in the case of QF NVs and MF VN-sequences and the absence of such a restriction in t1 NVs.[52]

To the great dismay of phonologists supporting some version of the PH, none of these types is phonetically motivated nor could it be—that is, if these researchers

[52]Note that even though the three t1 NVs in Mahu are ɛ̃ ɔ̃ ã—which may also occur as t2 NVs in Mahu—, the must-contain-A constraint could not select these and only these three vowels because Mahu also exhibits two NVs which contain A but which only occur as t2 and never as t1 NVs, *i.e.* ẽ õ.

are not subscribers to strategy 1 for the maintenance of the PH: denial. So it is not only the case—as I have shown in part 1 of this thesis—that 'known phonological feature specifications' (on the basis of which phonetically driven phonologists establish when phonetical motivation actually corresponds to attested measurements) are always motivated non-phonetically. Apparently, NVs can behave in at least four different ways, and no phonetician is to my knowledge able to predict or 'phonetically motivate' the occurrence or distributional properties of even one of these phonological NVs, let alone of all four of them. More specifically, not only does Huffmann have to admit that oral segments show nasal flow in oral contexts (*cf.* section 1.4.4), I have found that phonologically nasal vowels—domain-final t1 NVs in Jula—may be phonetically oral. This should make it abundantly clear to anyone that the phonological side of 'nasality' is not motivated by or grounded in phonetics. Let me add for those who would now go out and try to establish an articulatory or perceptual motivation for the attested 'nasal loss' in domain-final t1 NVs in Jula that t2 NVs occurring in the same language can, so it seems, be nasal without any phonetically motivated urge for 'nasal loss'; consequently, any phonetic explanation appears futile to the empirically minded researcher.[53]

In addition, since most languages with domain-final NVs have no articulatory or perceptual problems with them, such cross-linguistic counter-evidence against a phonetic explanation of domain-final nasal deletion in Jula would have to be ignored by supporters of the PH (denial, section 1.3.1) or be made ineffective by immunisation of the PH against falsification via non-application of the PH (strategy 2, 1.3.2) or by formulating a version of the PH that is in principle falsifiable but still ensures that most of the explanations based on it are not falsifiable (degrees of falsifiability, 1.3.3). It is of course a great shame indeed that progress in phonology is inhibited by the continuous reinstatement of the PH as a fixed idea, particularly because this uncritical approach is due to the (rarely discussed) opinion that the refutation of a theory is a sure sign of failure, not of progress. Let me therefore add the following quote from

[53]Based on data partly identical to the data referred to in this chapter but without presenting most of the details discussed here, Piggott [371] rejects the PH and proposes the Phonological Hypothesis. Similarly, in Piggott & Hulst [372] and Piggott & Humbert [373] Guaraní nasal harmony is analysed as a predominantly phonological phenomenon. Note also that Piggott's term 'Phonetic Hypothesis' used in [371] (1999) is identical to a term I employ in [382] (1997); Piggott [371] does not mention that the contents of his Phonological Hypothesis (nasality motivated by a cognitive system) are essentially claims made by Kaye [253] and other researchers working within GP. In addition, type B nasal harmony explained by Piggott ([371] (1999), [370] (1997)) as phonological (not phonetic) harmony at a syllabic level corresponds to spreading at the nuclear projection in Ploch [381] (1996). Furthermore, Piggott's analysis of type A nasal harmony as segmental harmony [*ibid.*] corresponds to spreading at the skeleton (Ploch [381]).

Popper which, I hope, demonstrates to those who still conduct phonetically grounded research that any scientific success is inevitably connected to refutation and that my criticism of the mainstream is meant in the most constructive way:

> "Refutations have often been regarded as establishing the failure of scientist, or at least of his theory. It should be stressed that this is an inductivist error. Every refutation should be regarded as a great success; not merely a success of the scientist who refuted the theory, but also of the scientist who created the refuted theory and who thus in the first instance suggested, if only indirectly, the refuting experiment" (Popper [392, p. 243]).

Conclusion

Having shown that the phonological behaviour of NVs cannot be explained phonetically but finds its motivation in the cognitively defined properties of phonology, let me add that there are still a number of questions that the approach put forward in this chapter cannot answer. These questions are related to the internal representations of the PEs involved and the licensing (and other) relationships holding between the skeletal positions dominating these PEs. For example, why are the lax non-high vowels a phonological class in Mahu and Jula? And why are the members of this class t1 NVs in Mahu but t2 NVs in Jula? Furthermore, I have referred to literature which shows that nasality in t1 NVs spreads or moves to the right in a number of languages. However, I have neglected in this chapter to discuss what the restrictions on (right-adjacent) consonantal targets for nasalisation are and how these constraints could be explained. I will discuss such issues in more detail in chapters 6 and 7.

Chapter 5

The Merger of L and N

In this chapter, I will first provide a brief description of the GP-internal history of the proposal to merge KLV's [265] L- and N-elements (section 5.1). I will show that this merger, even though commonly seen as established by GPists, has not been discussed in any detail in the relevant literature. To remedy this situation, I will subsequently, in section 5.2, present my arguments for the L/N-merger.

5.1 The theory-internal history of the proposed L/N merger

One common argument in favour of elimination of the N-element can be found in Cobb [108, pp. 41ff.]. In her opinion, the charmed version of N, *i.e.* N^+, is considered "troublesome" because, as pointed out above, charm theory outlaws all fusion operations of elements with identical charm. Since both N^+ and A^+ are positively charmed, it should be universally ungrammatical for these two elements to fuse. Strangely though, this is supposedly why N is a "candidate when it comes to reducing the numbers" of elements. Apparently, N was eliminated as part of a more general trend between 1990 and 1995 to reduce the number of elements employed within the ToE. The main reason for this reduction was that GP still predicted more PEs than were attested (*cf.* Cobb [110, pp. 38ff]). So problem-solving in relation to phonological melody was equated by the elimination of elements. This logic is unfortunately flawed since, along with charm theory, everything about N, which Cobb deems to be problematic, *i.e.* its apparent 'affinity' to A as opposed to the predicted 'phobia', had been abandoned. In other words, in the relevant GP literature, no reason for the elimination of the N-element is given. It is simply assumed (Cobb [110, p. 39], Ploch [380]) that N (or the low-tone element L) can, firstly, be abandoned by, secondly, merging it with L (or N, respectively).

In this context, Cobb points to evidence from Japanese in Nasukawa [334] to support the merger of L and N. Unfortunately, Nasukawa [334] is a reference to a paper presented at the LAGB Autumn Meeting (1995), which I have no access to; Nasukawa simply uses an N-element in his account of Gokana nasality and nasal harmony from the same year [335]. In [336], Nasukawa makes use of an idea which I attribute to Kaye [258]: to merge N with L; the only difference is that the result of the proposed merger is called 'N' by Nasukawa but 'L' by Kaye. So the innovation in Nasukawa's proposal is terminological. In line with Kaye [*ibid.*] who assumes an L-operator for nasal stops and an L-head for voiced stops, Nasukawa has an N-operator for nasal stops and an N-head for voiced stops. Note that, while Kaye bases his assignment of H/O roles on the inability of nasal stops to govern a recessive point in branching onsets (*cf.* (19) in chapter 4, p. 184), Nasukawa [336] contains no argument in favour of his H/O role distribution.[1] Let me also point out that, in one of his analyses, Nasukawa [336] has to derive nasal stops from underlying voiced ones. As I have pointed out in Ploch [382, 385] and in chapter 2, this is contrary to typological evidence: languages without alternations between nasal and voiced stops and with only one series of lexical voiced *or* nasal stops, always exhibit a series of nasal ones. Since Nasukawa does not even point to this typological problem, the distribution of head/operator roles of one and the same element for nasal and voiced obstruents as proposed by him appears to be an inkling.

All in all, we can say that, with the exception of Nasukawa [336] in 1997 and, in less detail but two years earlier, with the exception of Ploch [380],[2] the merger of N and L is not discussed. Because of this situation, I will in the following sections provide the evidence in favour of such a merger.

5.2 Merging L and N

In the following, I will provide four arguments in favour of merging KLV's [265] N- and L-element into one new element which I will refer to as 'L-element' (or

[1]I have used Kaye's proposal in Ploch [381, p. 102] where I mention "L-sharing between onset and nuclei" and in Ploch [382, p. 258] ("nasal sharing"). Both quotes refer to the idea that one manifestation of L, *i.e.* an L-operator in onset position (nasal stop), must be shared with—*i.e.* licensed by—an L-head dominated by its (following) nuclear licenser (nasal vowel). More recently, Kula & Marten [279] base their analysis of Bemba nasal-obstruent clusters on Nasukawa's, *i.e.* Kaye's, H/O role assignment.

[2]In Ploch [380, p. 92], I mistakenly give Piggott [369] as source of the data provided in favour of an L/N merger. The correct reference is Padgett [360] (*cf.* below, section 5.2.1).

'new L-element' where necessary); KLV's L-element I will call 'old L-element'. These four arguments involve postnasal voicing of voiceless obstruents, alternations between nasal and voiced stops dependent on the nasality/orality of the following vowel (nasal sharing, *cf.* (15) in chapter 2, p. 81), a unified account of voiced stops blocking high tone spreading and voiceless obstruents blocking nasal spreading (due to L/H-incompatibility) and and integrated approach to two dissimilation laws, *i.e.* Dahl's and Meinhof's Law.

5.2.1 Postnasal voicing of voiceless obstruents

There are a number of cross-linguistically common processes or distributional patterns, *e.g.* in Ecuador Quichua, Terena, Zoque, Kpelle, Campa, Yamato Japanese, Kikuyu, Luyia and Korean, in which voiceless stops and/or fricatives become voiced when following nasal stops or where all obstruents following nasal stops are obligatorily voiced. Since voicing is motivated by the presence of a low tone element (*cf.* Harris [198, p. 264]), Kaye [258] suggests fusing N and L.

Before I propose my account of postnasal voicing, let me first provide the relevant examples.[3] According to Orr's [356, p. 74] phonological sketch of Ecuador Quichua, "Morphemes whose alternants otherwise begin with a voiceless stop have an allomorph beginning in a voiced stop of the same quality when following a nasal"; that nasal is homorganic with the (following) suffix-initial consonant:

(1) *Ecuador Quichua*

-pa	'*genitive*'	sinik-pa	'porcupine's'	kam-ba	'yours'
		ʧilis-pa	'streamless region's'	hatum-ba	'the big one's'
-pi	'*locative*'	saʧa-pi	'in the jungle'	hatum-bi	'in the big one'
		punʤa-pi	'in the daytime'	atam-bi	'on the frog'
-ta	'*object*'	wasi-ta	'the house'	kan-da	'you'
		puru-ta	'gourd'	atan-da	'frog'
-ʧu	'*question*'	ali-ʧu	'is it good?'	kan-ʤu	'you?'
		lumu-ʧu	'manioc?'	tijan-ʤu	'is there?'

[3]Padgett [360] discusses the Zoque and Kpelle data presented below in terms of feature geometry in a way such that her analysis is "more in line with models based on facts of phonetics and vocal tract anatomy, e.g., the gestural model of Browman and Goldstein" [69, 70, 71]. How unfalsifiable this view is, I have shown in part 1 of this thesis.

In Terena, a language spoken in south-western Mato Grosso (Brazil), in forms denoting the first person singular a nasalising agent is attached to the first onset of a domain and spreads rightwards. Note that nasal stops only trigger nasal harmony in first person singular forms. Where nasal spreading reaches a voiceless stop or fricative, "a nasalized consonantal sequence replac[es] the first stop or fricative in the word [including word-internal stops and fricatives] as follows: mb replaces p, nd replaces t, ŋg replaces k, nz replaces both s and h and nʒ replaces both f and hy [^{h}y]" (Bendor-Samuel [40, p. 350]); the resulting prenasalised voiced stops block further spreading of N to the right:[4]

(2) *Terena*

3sg	*1sg*	*Gloss*
owoku	õw̃õ^{ŋ}gu	'house'
nokone	nõ^{ŋ}gone	'need'
otopiko	õ^{n}dopiko	'chopped'
iwatako	ĩw̃ã^{n}dako	'sat'
tuti	^{n}duti	'head'
piho	^{m}biho	'went'
simoa	^{n}zimoa	'came'
haʔa	^{n}zaʔa	'father'
ʃeʔeʃa	^{n}ʒeʔeʃa	'son'
a^{h}yaʔaʃo	ã^{n}ʒaʔaʃo	'desire'

Zoque, spoken in Copainalá (Chiapas, Mexico), exhibits a nasal prefix (first singular possessive) which is homorganic with the following stem-initial consonant. However, stem-initial voiceless stops become voiced in this context, the nasal prefix is always n before w j h and is lost before ʔ f s ʃ m n ɲ l r (*cf.* Wonderly [506, pp. 120f.]). Examples with stem-initial stops are:[5]

[4]For data, *cf.* Bendor-Samuel [40].

[5]For further information on Zoque, *cf.* Wonderly [502, 503, 504, 505, 507, 508, 509, 510]

(3) *Zoque*

Stem	*1sg.poss*	*Gloss*
pama	mbama	‘clothing’
bur'u	mbur'u	‘burro’
tatah	ndatah	‘father’
disko	ndisko	‘phonograph record’
ʧoʔngoya	ɲʤoʔngoya	‘rabbit’
kaju	ŋgaju	‘horse’
gaju	ŋgaju	‘rooster’

In Kpelle, a language spoken in central Liberia and adjacent parts of Guinea, the first singular prefix, *i.e.* a syllabic and tone-bearing nasal, turns a following stem-initial voiceless stop or fricative into its corresponding voiced version; the nasal is homorganic with the stem-initial consonant (*cf.* Welmers [491, 493]):[6]

(4) *Kpelle*

Stem	*1sg.poss*	*Gloss*
pôlu	ḿ-bôlu	‘back’
tía	ń-día	‘taboo’
kɔ́ɔ	ŋ́-gɔ́ɔ	‘foot’
k͡pîŋ	ŋ́-g͡bîŋ	‘myself’
féla	ḿ-véla	‘wages’
sṹa	ń-ᵈzṹa	‘nose’

Campa is spoken in eastern central Peru (*cf.* Dirks [138]). Voiceless stops (p t c k) become voiced (b d ɟ g) after nasal stops in Dirks's analysis [*ibid.*, p. 302]; voiceless affricates (ʦ ʧ) are unaffected by preceding nasal stops [*ibid.*]. Note, however, that the occurrence of voiced stops is restricted to clusters of ‘nasal stop plus (voiced) stop’; in other words, stops are always voiceless unless they follow nasal stops.[7]

[6]In my copy of Welmers [491], it is not clear whether the first vowel in the Kpelle word for ‘taboo’ is nasalised or not (tía *versus* tĩ́a).

[7]I have substituted Dirks's symbols tʸ dʸ c č ñ š y for IPA symbols, *i.e.* c ɟ ʦ ʧ ɲ ʃ j, respectively. Also note that “in the second syllable of disyllabic words in which stress falls on the first syllable this variation [‘turning’ voiceless stops into voiced ones] does not occur” [138, p. 303]. Dirks's example is ˈiŋki *ˈiŋgi ‘peanut’ [*ibid.*].

(5) *Campa*

kombir'oʃi	*kompir'oʃi	'a palm leaf'
nih'anda	*nih'anta	'far away'
niʃ'iɲɟo	*niʃ'iɲco	'my daughter'
kir'iŋga	*kir'iŋka	'downstream'
iŋg'ani	*iŋk'ani	'rain'

Furthermore, in Yamato Japanese morphologically simplex forms there are no nasal-obstruent sequences with voiceless obstruents (6a.) (*cf.* Nasukawa [336, pp. 404f.]). Similarly, in morphologically complex forms (but not in Sino-compounds), a stem-final nasal assimilates to a following suffix-initial obstruent in relation to place of articulation, while that obstruent, if underlyingly voiceless, becomes voiced (6b.):[8]

(6) *Yamato Japanese*

a. morphologically simplex forms

ʃombori	*ʃompori	'discouraged'
ʃindoi	*ʃintoi	'tired'
kaŋgae	*kaŋkae	'thought'
koŋgari	*koŋkari	'done to a golden brown'

b. morphologically complex forms

Stem	*Gerundive*	*Past indicative*	*Alternative*	*Subjunctive*	*Gloss*
	-te	-ta	-tari	-tara	
ʃin	ʃin-de	ʃin-da	ʃin-dari	ʃin-dara	'die'
kam	kan-de	kan-da	kan-dari	kan-dara	'chew'

Herbert [213, pp. 66f., 236f.] states that many languages only exhibit a series of nasal-obstruent clusters with voiced but not one with voiceless obstruents. He points to Kikuyu, Swahili, Malagasy, Luyia and Kamba—without the relevant references[9]—and provides the following transformation rules for Kikuyu [*ibid.*, p. 66] and Luyia [p. 236]:

[8]As elsewhere, I employ IPA symbols, so Nasukawa's š has been substituted with ʃ.

[9]In Herbert's bibliography, I found the following references with titles containing 'Kikuyu' or 'Luyia': For Kikuyu, Armstrong [18], Barlow [28], Myers [333], Sharp [444]; for Luyia, Appleby [13].

(7) *Kikuyu and Luyia*

Kikuyu	*Luyia*
N + t → nd	/N + p, t, k, ts, c/ → [mb, nd, ŋg nz ɲɟ]
N + c → ɲɟ	
N + k → ŋg	

Finally, consider the following Korean data taken from Heo [212, pp. 137, 149]:[10]

(8) *Korean*

a. Neutral obstruent → voiced obstruent / nasal stop _

s°oːŋgot°	'gimlet'
t°oŋbæ	'dividing'
ʧ°aŋdori	'hammer'
s°iːmburɨm	'errand'
namʤit°	'over'
ʧ°amgi	'to sink'
mundɨk°	'suddenly'
t°angol	'customer'
s°ənbi	'scholar'

b. Postnasal tensed or aspirated obstruents

mæːŋk'oŋi	'fool'
p°ənʧ'ək°	'sparkling'
t°oːŋʧʰimi	'pickled cabbage'
k°əːmʧʰi	'to attach'
əːmpʰo	'threat'
oːntʰoŋ	'whole'

The Korean lexicon exhibits the following consonants: neutral obstruents: p° t° k° ʧ° s°, tensed obstruents: p' t' k' ʧ' s', aspirated obstruents: pʰ tʰ kʰ ʧʰ, and m n ŋ r h. Relevant to our discussion of postnasal voicing is that there are no lexical voiced stops in Korean and there are no sequences 'nasal stop + neutral obstruent' (NC°). In can therefore be said that one of three types of voiceless obstruents, *i.e.* the neutral obstruents, turn into their corresponding fully voiced version when following a nasal stop.[11] Postnasal tensed or aspirated obstruents not affected.

[10] In opposition to the usage of 'c' as IPA symbol for a voiceless palatal stop, c° c' cʰ refers in Korean linguistics to affricates. To be consistent within this thesis, I will use 'ʧ° ʧ' ʧʰ' (respectively) instead.

[11] In section 7.1, I will put forward an explanation for this phenomenon.

In view of this evidence, I conclude that there is a cross-linguistic pattern in which voiceless obstruents become voiced when adjacent to nasal stops. Assuming that voicing is motivated by the presence of an L-element, this situation points to a conflation of old N with old L into one and the same element (new) L.

5.2.2 Nasal licensing

Having looked at postnasal voicing, the next example of a correlation between nasal and voiced stops I would like to point out is 'nasal licensing' (which I called "nasal sharing" in Ploch [382, p. 258]). Consider the following data from Cubeo, an Eastern Tucanoan language spoken along the middle Vaupés River in south-eastern Colombia (*cf.* Salser [425]):[12]

(9) *Cubeo*

kõl̃ẽ́kihiwɨ	'I am going to urinate'	h̃ĩ́^{n}dɨ	'egg'
kolékihiwɨ	'I am going to wait'	mĩmĩ́ɟo	'hummingbird'
kuinã́l̃ṍ	'one'	ɨ̃ɨ̃́ɟoka	'his leaf'
koló	'species of bird'	ãð̃ã́	'snake'
^{m}bákó	'mama'	h̃ɨ̃hólɨ́	'squirrel'
ɟ̃ãmã́ko	'deer'	mũĩ́ɟoka	'*caraná* (Portuguese) leaf'
^{n}dahákɨ	'come!'	hiáð̃okũ	'canoe'
nãh̃ṍkó	'shrimp'	kámṹká	'ear'
^{m}boawɨ́	'I killed'	kámãwe	'batting'
kɨ̃́^{m}bá	'comb'	h̃ã́ɟ̃ɨ̃mũ	'I am looking at'
^{n}dúdukɨ	'chigger'	ãíɟe	'food'

As the data in (9) illustrate, nasality does not spread through a domain in Cubeo. For example, ɟ̃ãmã́ko shows that ɟ can be nasalised (ɟ̃). However, in mũĩ́ɟoka, ɟ is not nasalised by the preceding NV ĩ́. Similarly, in ãíɟe, í is not nasalised by preceding ã. This excludes rightward harmony. n in kuinã́l̃ṍ does not nasalise preceding i; m in kámãwe does not nasalise á. This makes the proposal of leftward nasal harmony unfounded. There is however a constraint on Cubeo syllables in relation to their nasality/orality status. While voiceless stops may be followed by oral or nasal vowels (k in kɨ̃́^{m}bá *versus* ^{m}bákó), nasal stops and nasal glides must be followed by

[12] Salser has [ň] for l̃, Ỹ for Ṽ, y for ɟ. Salser's y is pronounced [^{n}j ∼ ^{n}d^{j}], not as a glide; what this means in phonological terms is beyond the scope of this discussion. Also, voiced stops are prenasalised in certain contexts; such prenasalised voiced stops "occur utterance initial and following contiguous nasal vowels word and utterance medial" (Salser [425, p. 76]).

nasal vowels, voiced obstruents (prenasalised or not) and oral glides by oral vowels. Note that it can be observed universally that in languages with no nasal sharing or other nasal harmony types the presence of a series of voiced stops implies a series of nasal stops, but not *vice versa* (*cf.* section 2.5.2, p. 78ff.). I propose therefore that, universally, in languages with nasal sharing, *i.e.* with only one series of either voiced stops or nasal stops, there is only a lexical series of nasal stops. We can analyse this phenomenon as 'L-licensing'. More specifically (and as I will discuss in more detail in chapter 7), a lexical L-operator attached to an onset (motivating a nasal stop) must be licensed by a nuclear L-head (motivating a nasal vowel); if the consonantal L-operator does not receive this license (because the following vowel is oral) it switches its head/operator status and becomes head (motivating a voiced stop).

Again, we find that nasality (motivated by old N) and voicing (motivated by old L) appear to correspond phonologically to one and the same element: new L.

5.2.3 L/H-incompatibility

It is well-known that in a number of Bantu languages, *e.g.* in Zulu (*cf.* Clark [98], Downing [142], Khumalo [272], Strazny [464], Traill, Khumalo & Fridjhon [478]), an H-tone otherwise assigned to a nucleus (N_1), is displaced one nucleus to the right (N_2) if the onset immediately preceding N_1, *i.e.* O_1, dominates a voiced stop (depressor-induced H-displacement):

(10) *Zulu depressor-induced* H-*displacement*

a. ú + ya + let^h + él + a → úyalet^{h}éla '(s)he is bringing for'
3sg.subj. pres. bring:benef. asp.

b. zí + ya + let^h + él + a → zìyálet^{h}éla 'they are bringing for'
3pl.subj. pres. bring:benef. asp.

While in (10a.) the nucleus of the third singular subject marker is lexically assigned an H-tone (ú in úya, with no tone—*i.e.* with mid tone—on ya-), the L-head in the depressor consonant z in the onset of the third plural subject marker in (10b.) spreads to the first available nucleus (N_1) and pushes the H that would otherwise be linked to N_1 to the next nucleus to the right (N_2), resulting in zìyá (with a H-tone on yá-).

Low tone and high tone/pitch are motivated in GP by the presence of an L- or H-operator within a PE that is dominated by a nucleus. The reason for this head/operatorship assignment, *i.e.* for not assuming that low or high tone correspond to an L- or H-*head* within a nuclear PE, is that, cross-linguistically, ATR-harmony operates completely independently of the tonal system of a language. Since in languages with systematic vocalic tense-lax contrasts, tense vowels are analysed as headed nuclear PEs and lax vowels as headless PEs (*cf.* Kaye [262] on English, Ploch [379] on German vowels), ATR-harmony is seen as head-alignment—resulting in phonetic 'tensification' (Cobb [109, 110], Kaye [258], Walker [485]). If phonetic high or low tone were due to to H- or L-tone as heads of nuclear PEs, all toned vowels would be predicted to be headed and thus to be triggers of ATR-harmony. Since tone has no influence on ATR-harmony, GP assumes phonetic tones to be motivated by an H- or L-operator. This means that the depressor-induced H-displacement shown in (10) cannot be explained by pointing to the universal constraint on PEs that they may only contain up to one head (*cf.* constraint 3 in (4), p. 163). It is true that if H- or L-heads *were* the phonological motivation for phonetic high or low tone/pitch, a PE could only be high- *or* low-toned because it could only contain either an H- *or* an L-head. However, since we must assume on the basis of an analysis of ATR-harmony as h-licensing that phonetic tones are the manifestations of nuclear H- or L-operators, the displacement of H in (10) can only be explained if we assume additionally—*i.e.* independently—that H and L are incompatible within one and the same PE.[13]

If nasality were motivated cognitively by a low tone element we would also expect to find evidence for incompatibility between high tone and nasality—without this implying that universally, H and L do not fuse. Neglecting languages which exhibit a contrast between mid-toned (*i.e.* no tone) NVs and low toned NVs,[14] such evidence is available. Consider the following data from Warao (*cf.* (14) in chapter 2, p. 79; data from Osborn [358]):

[13] I neglect here whether this incompatibility is universal or merely cross-linguistically common.

[14] I will claim in chapter 6 that the simultaneity of low pitch and nasality in such cases in only phonetical, not phonological.

(11) *Warao (type A): nasal harmony blocked by voiceless stops*

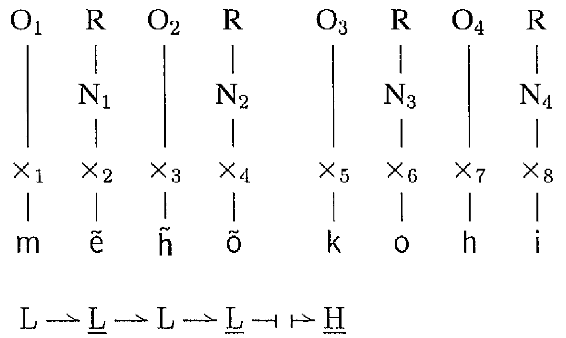
inãw̃ãh̃ã	'summer'
mõj̃õ	'cormorant'
hĩh̃ã	'kind of bird'
mõãũ	'give it to him!'
mõãũpu	'give them to him!'
mẽh̃õkohi	'shadow'
tewẽke	'kind of bird'
terẽ	'it broke'
jã	'walking'
etere	'bell'
pajara	'sword fish'
hiha	'your hammock'
oi	'look out!'
ja	'sun'

As I will explain in more detail in chapter 7, in some languages which display nasal harmony the spreading of nasality—left to right in Warao—is blocked by voiceless stops (or obstruents). There is evidence from analyses involving constituent government in many Indo-European languages, inter-onset licensing in Korean, final obstruent devoicing in (some varieties of) German and in Turkish and devoicing/tensification in English and Augsburg Swabian (*cf.* chapter 7) that (non-neutral) voiceless stops contain an H-head. Furthermore, since nasal stops are universally ill-formed constituent governors, *i.e.* they do not occur as lefthand members of branching onsets, nasality in consonants is seen as phonetic manifestation of an L-operator present in a PE attached to a non-nuclear point.[15] Assuming that nasality is motivated by an L-element, L spreading rightwards would be expected to try to link to a target onset position as operator. In other words, without an incompatibility constraint for H and L, there would be no reason why an L-operator would not spread into a PE containing an H-head. Now consider the following representation of Warao

[15]Note that nasality in vowels has to be motivated by a nuclear L-head because nuclear L-operators are phonetically realised as low tone/pitch. Kaye [258] refers to this antagonistic distibution of head and operator roles between onsets and nuclei as 'ying-yang principle'. Polish I-sharing between an onset and its licensing nucleus provides independent evidence for the ying-yang principle (*cf.* Kaye [*ibid.*]).

mẽh̃õkohi (from (11)):

(12) k *in Warao* mẽh̃õkohi *'shadow' blocks rightward spreading of nasality*



Apparently, k contains an H-head and blocks an incoming L. Let me add though that I will propose in chapter 7 that even though L spreads from consonants containing it as operator (*i.e.* from nasal stops) it will in many languages try to become head of the target consonant. However, in languages exhibiting type B nasal harmonies (where voiceless obstruents do *not* block the spreading of nasality) L harmonises by attaching to consonantal target PEs as operator, and it behaves in such-like manner even if the target PE contains H, thus harmonises voiceless stops (which contain an H-head). This means that there is evidence which suggests that H and L are incompatible with each other within one and the same PE but that this incompatibility, even though it is cross-linguistically common, is not universal. Together with the H/L-incompatibility evident in cases of depressor-induced H-displacement, we can say that we can explain both types of incompatibility by the assumption that nasality is motivated by the same element that displaces H and is realised as voicing in consonants, *i.e.* by the L-element.[16]

Further evidence can be found in Zulu (*cf.* Strazny [464]) where, similar to nasal harmony being blocked by H in onset position, left to right H-spreading (from nuclear sources) is blocked by voiced stops and (sometimes) by nasal stops, *i.e.* by L linked to an onset—in the case of voiced stops as head, for nasal stops as operator.[17]

All in all, we can say that the parallelisms between voicing and nasality in rela-

[16]This analysis can only work if we assume that h, which occurs in Warao as oral segment and in its nasalised form (h̃), does not contain an H-element. Since h is a bad constituent governor in most languages with branching onsets and a bad inter-onset governor in Korean, I do not see this view as problematic.

[17]*Cf.* Kaye [251], Kaye, Koopman & Sportiche [264] on the blocking of H-spreading by voiced stops in certain Kru languages (*e.g.* Vata or Dida). Similarlyly, in Applecross Gaelic (*cf.* van der Hulst & Smith [230, pp. 317ff.], Ternes [472]), s and f, *i.e.* voiceless fricatives containing an H-operator, but not voiceless stops (containing an H-head) may be nasalised.

tion to their apparent PE-internal incompatibility with the presence of an H-element provide further evidence for the merger of KLV's [265] N and L into new L.

5.2.4 Laws of dissimilation: Dahl's and Meinhof's Law

There are a number of 'famous' laws stating cross-linguistic (typological) dissimilation pattern, *e.g.* Dahl's, Lyman's, Meinhof's and Grassmann's Law. I will show in this section that (at least) Dahl's and Meinhof's Law can be captured by a unified account if we assume the merger of old L and N into new L. Let me discuss each of these two laws in turn.

5.2.4.1 Dahl's Law

I will start with Dahl's Law. Dahl's Law is, as I will show, not about nasality. Meinhof's Law, however, is. Importantly, I can explain both laws by the (almost identical) restriction on inter-onset licensees not to dominate PEs containing an H- (Dahl) or L-head (Meinhof).

In Kikuyu, a Kenyan Bantu language, there is according to Nasukawa [336, p. 406] a constraint which restricts the occurrence of voiced consonants.[18] The following data is from Clements (*cf.* Halle & Clements [197, p. 107]):[19]

[18]Nasukawa refers to Armstrong [19], Davy & Nurse [129], Pulleyblank [402], Rice [409]. There is a book by the same Lilias E. Armstrong with the same title [18] but published 27 years earlier, *i.e.* in 1940.

[19]I have changed Clements & Halle's y into j.

(13) *Dahl's Law in Kikuyu*

a. ko-		b. ɣo-	
koina	'to dance'	ɣoita	'to strangle'
kooria	'to ask'	ɣotɛŋɛra	'to run'
komɛɲa	'to know'	ɣokuua	'to carry'
koniina	'to finish'	ɣokoora	'to root out'
kohɔta	'to be able'	ɣokuɲa	'to pinch'
kohetoka	'to pass'	ɣokaja	'to cut into strips'
kohɛ	'to give'	ɣocina	'to burn'
koɣeera	'to fetch'	ɣocuuka	'to slander'
koɣamba	'to make a sound'	ɣoðɛka	'to laugh'
koɣuuta	'to comb'	ɣoðaaka	'to play'
koɣaja	'to divide out'		
koruɣa	'to cook'		

In Nasukawa's view [*ibid.*], examples like those presented in (13) show that the ɣ-initial prefix precedes syllable-initial voiceless consonants while the k-initial variant is chosen for stems beginning with a voiced consonant. Since he states in the same article that his analysis is based on Element Theory, I find this claim surprising. Consider the following quote:

> "In [(13a.)], the initial consonant of the prefix is voiceless, since the stem-initial consonant is voiced, and therefore, the structural conditioning for Dahl's Law is not met. Note that in ... *ko*-**n***iin-a* ..., nasals are treated as voiced; in other words, nasals seem to require voice in their internal structure in the light of Dahl's Law" (Nasukawa [336, p. 406]).

So voicing in the stem-initial consonant corresponds to the structural conditioning for Dahl's Law not being met, resulting in the voiceless version of the prefix onset (k). In other words, Nasukawa defines as natural class all voiceless stem-initial consonants because they trigger the condition for Dahl's Law being met, resulting in the voiced version of the prefix. Firstly, let me point out that ɣoita in (13b.) (with t triggering prefixal ɣ (not k) as well as in koina and kooria in (13a.) (with 'voiced' n or r not triggering prefixal ɣ) illustrates that it is not, as Nasukawa assumes, the stem-initial consonant which is relevant but the leftmost (filled) onset position in a domain. For example, since in ɣoita the stem-initial onset is empty, t is the consonant taken into account. In Nasukawa's non-sensical analysis the form of the prefix contains underlying k when preceding stem-onsets which belong to one and the same natural class (*i.e.* voiced stem onsets) while he has to define the triggering context for Dahl's Law as a [−voice]-specification for stem onsets. Since Nasukawa employs [−voice]—or, possibly, for him the absence of new N—to define the stem onsets which correspond to prefixal ɣ, he apparently, without any discussion, changes a privative element, *i.e.* (new merged) N—new L for me—into a binary feature where, *opposed* to Element Theory, also the absence of an element can be used to define a natural class. As pointed out in chapter 2 (section 2.1, pp. 57), binary features tend to overgenerate. However, this problem does not even occur to Nasukawa.

That Nasukawa is mistaken in analysing all stem-initial consonants which display the ɣ-initial prefix as natural class or, alternatively, in regarding both the stem-inital consonants in (13a.) and (13b.) as (distinct) natural classes becomes evident if we look at kohɔta, koɣeera and koruɣa, basically all stems in (13a.) which do not start with a nasal stop: In Element Theory, none of h ɣ r are (usually) assigned an L-head (motivating voicing in obstruents). For example, in the above mentioned cases of depressor-induced H-shift (*cf.* section 5.2.3), Nasukawa's 'voiced' consonants h ɣ r do not trigger this kind of displacement. It follows that Nasukawa works within some version of Element Theory but does not provide any independent phonological evidence for assigning r an L- (or N-) element. Most likely, Nasukawa is simply phonetically misguided and translates phonetic voicing into some element. How futile any version of the PH is (even this tacit and probably unwitting one) I have shown in part 1 of this thesis. So if Nasukawa thinks that the data in (13a.) provides evidence for a link between nasal and voicing, he is on the wrong track. Note however that even though r and v may contain an L-element in some languages, I will argue below that this does still does not make it possible to define as natural class all cross-linguistically observable stem-initial PEs which have prefixal k in Dahl's Law contexts.

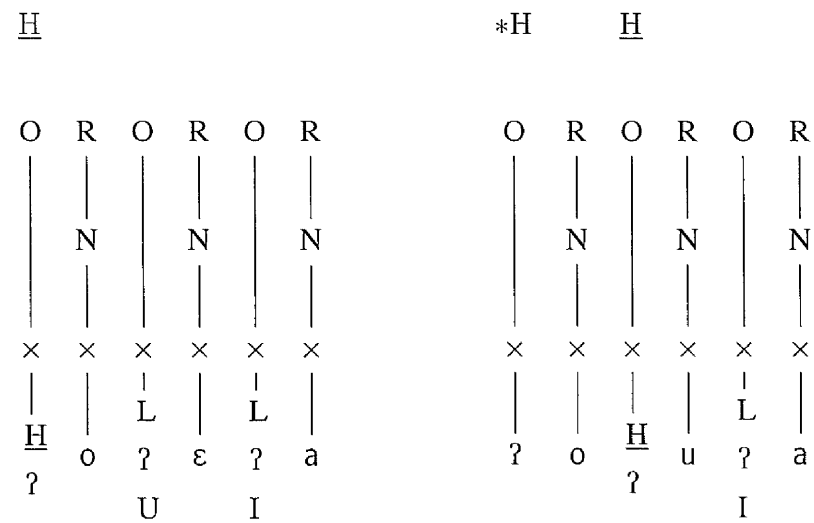
Taking into account Harris [198, 200, 202], Harris & Lindsey [204, 205], KLV [265], Kaye [258] and very importantly, Heo's work on Korean [212], to name but a few, it is clear that in Element Theory, the leftmost stem-consonants in (13a.) do not form a phonological class while those in (13b.), *i.e.* k t c ð, do in that they all exhibit an H-element or, possibly more specifically, an H-head. That is to say that I propose that the underlying (lexical) representation of the PE dominated by the infinitive prefix-initial onset point is (ʔ·$\underline{\text{H}}$) (k). Since it is the onset of the prefix which is in certain circumstances altered by H-switching—resulting in derived ɣ (ʔ · H)—, let us say that the prefixal onset dominating k (ʔ · $\underline{\mathrm{H}}$) is scanned via a head-final (right-to-left) licensing relationship at the onset projection from the point dominated by the leftmost filled stem onset (licensor) and the prefixal onset point (licensee). This relation is subject to a dissimilation constraint: an H-head cannot be licensed by an onset which also contains and H-head. An unlicensed H-head switches its H/O ('head/operator') role and becomes operator (14b.). Of course, if the leftmost stem onset dominates one of m n r h ɣ (not containing an H-head), the conditions for dissimilation are not met and lexical k remains unchanged at PL (14a.).

Let me add that one might try to say that the motivation for Dahl's Law does not lie in H-switching but in H-deletion. So we could say that an H-head (or possibly, H-element) is delinked when unlicensed (*i.e.* when the potential licensing onset dominates H (or an H-head) itself. As my analysis of Meinhof's Law in section 5.2.4.2 will show, in cases where adjacency not of H but of L is subject to dissimilation, it cannot be claimed that an L-element (or -head) is delinked when unlicensed since the lexical PEs subject to dissimilation, *i.e.* prenasalised voiced stops (mb (ʔ · U · $\underline{\mathrm{L}}$)), do not turn into neutral stops (b/p (ʔ · U)) but into nasal stops (m (ʔ · U · L)).

To sum up, since I want to propose a unified account of H- and L-related dissimilation, I have to propose that in both cases, a H/L-head may not be licensed by an identical head and that the method to avoid the unwanted clash is H/O role switching of H/L from head to operator. There is one piece of evidence in this argumentation that I have not taken into account yet: as pointed out in chapter 4, ʔ and H only co-occur when H is head. Even though this restriction does not follow from any principle, there is no such constraint on L when co-occurring with ʔ: *e.g.* in m (ʔ · U · L). So we can maintain a unified account for Dahl's and Meinhof's Law. In the case of Meinhof's Law, an L head (co-occurring with ʔ) switches its lexically assigned H/O role to operator to avoid two consecutive L-heads. This option is not available in the case of Dahl's Law. Since k (ʔ · $\underline{\mathrm{H}}$) with H switching its H/O role would result in (ʔ · H), a PE excluded universally, the only way to dissimilate the underlying H-head is to delink it. The following representations illustrate H-dissimilation:

(14) *Kikuyu* komɛɲa, ɣokuɲa

a. komɛɲa *(at LL/PL)* b. ɣokuɲa *(at PL)*



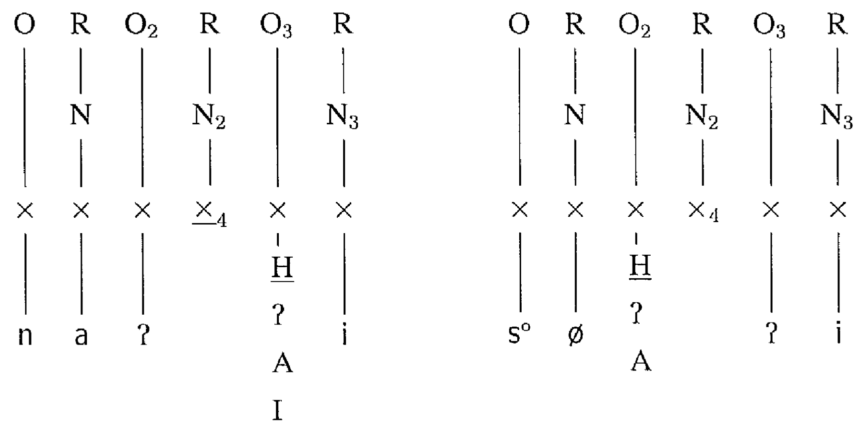
Note that my analysis is backed up by Heo's account of head-final onset-to-onset government in Korean. In Korean, inter-onset government forms one the conditions on p-licensing. So in a series $O_1N_1O_2N_2$ (dominating $C_1V_1C_2V_2$, respectively), N_2 can only p-license N_1 if (amongst other conditions) the onset licensee of N_2, *i.e.* O_2, can onset-to-onset govern the onset licensee of N_1, *i.e.* O_1.[20] Relevant to our discussion is that, in an updated version of the Theory of Elements, Heo would have to assume that (word-internal) C_1-licensees must not contain a H-element (or, possibly, H-head) if N_1 is to be p-licensed. This excludes p' t' k' ʧ' s', pʰ tʰ kʰ ʧʰ (with H-head, *cf.* napʰɨ̠tʰallin 'naphthalene', *napʰtʰallin [212, p. 113]) in C_1 position when preceding a p-licensed empty nucleus (N_1) but does not, *ceteris paribus*, disallow the neutral stops p° t° k° which contain no H-element at all (*cf.* s°æːk°s'i 'girl', *s°æːk°ɨ̠s'i [212, p. 149]).[21]

[20] As pointed out in section 4.1.3, the leftmost nucleus in a domain cannot be p-licensed (silent) in Korean. So the index numbers in $O_1N_1O_2N_2$ are positional variables and could, with p-licensed N_1, be filled by $O_2N_2O_3N_3$, $O_3N_3O_4N_4$, *etc.*, but not by $O_1N_1O_2N_2$.

[21] *Cf.* my summary of the RET in chapter 4. Opposed to Korean, Kikuyu H-dissimilation does allow H-heads to be licensed. In the case of Dahl's Law, only an H-head may not license an H-head. Note also that Heo's 'ö' is given as 'ø'. To remind the reader, Korean exhibits a three-fold contrast for for obstruents other than h: in Heo's terms, there are neutral consonants (p° t° k° ʧ° s°), tensed (p' t' k' ʧ' s') and aspirated ones (pʰ tʰ kʰ ʧʰ). Note that there is no aspirated *sʰ.

(15) *Korean* nak°ʧ'i, s°øt'ɨk°i *(at PL)*

a. nak°ʧ'i b. s°øt'ɨk°i



For example, in both nak°ʧ'i 'octopus' (15a.) [*ibid.*, p. 101] and s°øt'ɨk°i 'a horse-tail' (15b.) [p. 113], domain-final i is adjacent to an empty nucleus (N_2 dominating $\times_4$) preceding i at the nuclear projection, and i is not itself licensed. Additionally, there is no branching rhyme or onset, *i.e.* a governing domain, intervening between proper governor (N_3) and the empty nucleus (N_2); N_3 is not a government-licenser. Consequently, all conditions on p-licensing (as set out in Charette [82, 83], KLV [267]) are met. However, while the empty nucleus between k° and ʧ' in nak°ʧ'i is p-licensed and silent as predicted, the empty nucleus intervening between t' and k° in s°øt'ɨk°i is not p-licensed and audible. The reason for this is that while ʧ' can inter-onset govern k° from right to left in nak°ʧ'i, k° cannot govern t' in s°øt'ɨk°i because H is ungovernable. Since one of the conditions on p-licensing, *i.e.* inter-onset government, fails, p-licensing government cannot take place in s°øt'ɨk°i.

Note that the non-existence of s° (A · H) in C_1 preceding p-licensed N_1 does not indicate whether H in any head/operatorship role or only when it is head cannot be licensed. This is due to another constraint on C_1, *i.e.* that it must contain a ʔ-element. Also, it is not possible to explain the fact that that k° (O_3) cannot onset-to-onset govern t' (O_2) by referring to the Complexity Condition (*cf.* Harris [198, pp. 273ff.]), *i.e.* the constraint that a governor (k° (ʔ) in s°øt'ɨk°i) may not be less complex—*i.e.* contain fewer elements—than its governee (t' (ʔ · A · $\underline{\text{H}}$)).[22] Even though t' is, in fact, more complex than k°, the Complexity Condition could not explain why tensed and aspirated consonants can never be inter-onset governees in Korean, *i.e.* even if

[22]Kaye [258] formulates a substantive constraint on transconstituent government according to which a governor may not be less complex than its governee; also, *cf.* Harris [199, pp. 385ff.], [203, pp. 347], KLV [267].

the governor is more complex than the assumed tensed or aspirated governee. So there are no examples where tensed kʼ (ʔ · $\underline{\mathrm{H}}$) is inter-onset governable by more complex PEs (*e.g.* pʼ (ʔ · U · $\underline{\mathrm{H}}$), tʼ (ʔ · A · $\underline{\mathrm{H}}$), ʧʼ (ʔ · I · A · $\underline{\mathrm{H}}$), $\mathrm{p^h}$ (ʔ · U)($\underline{\mathrm{H}}$), $\mathrm{t^h}$ (ʔ · A)($\underline{\mathrm{H}}$), ʧʰ (ʔ · A · I)($\underline{\mathrm{H}}$), ʧ° (ʔ · A · I)) or by equally complex PEs (*e.g.* sʼ (A · $\underline{\mathrm{H}}$), s° (A · H), p° (ʔ · U), t° (ʔ · A)), all of which would be predicted on the basis of the Complexity Condition to be well-formed governors for k°. It appears that the relevant constraint restricting the shape of PEs dominated by inter-onset governees refers to the ill-formedness of a PE containing H in this context.

In relation to Kikuyu ð which I have assigned an H-head in my analysis, let me point out here that in English (and other languages with branching onsets) H-headed PEs are good constituent governors (*e.g.* English p t k f) while those displaying an H-operator, *e.g.* s, are not. It must be left to future research for the purposes of our discussion here whether English ð—with its domain-inital restriction to closed class categories ([ð]is, [ð]ose, [ð]ere)—or ð in any other language contains H and if so whether as head or operator. What can be said though is that Kikuyu ð, because it triggers H-head dissimilation, must contain an H-head.

I conclude that Dahl's Law falls out of the version of Element Theory adopted here if we propose in line with Heo's work on Korean that in a language in which Dahl's Law is operative, inter-onset licensees dominating a PE containing an H-element are subject to certain restrictions. Korean and Kikuyu differ with respect to the precise nature of the restriction in question. While in Korean, H in general is not onset-to-onset governable, in Kikuyu only H-heads are constrained. This constraint precludes H-heads being inter-onset licensed by an H-head. Nasukawa's assumption that Dahl's Law has anything to do with voicing or the element motivating some instances of voicing (new L for me, new N for him) is mistaken.

Even though this is usually not mentioned when Dahl's Law is discussed, there is a similar phenomenon in virtually all so-called varieties of German spoken in the South of Germany, Austria and Switzerland. The variety I will use to illustrate this point is my native tongue: Augsburg Swabian ('AS'), spoken in—the city of Augsburg, on the Western, *i.e.* Swabian, side of the dialect border between Swabia and Bavaria. Consider the following data (mine):[23]

[23]The selection of verbs is meant to be representative in relation to possible domain beginnings.

(16) *Dahl's Law in Augsburg Swabian verbs*

a. past participle prefix k-

Infinitive	*1sg present*	*Past Participle*	*Gloss*
faŋŋ̍	faŋ	kfaŋŋ̍	'catch'
frɛsn̩	frɪs	kfrɛsn̩	'guzzle *(food)*'
flʲiːkŋ̍	flʲiːkʰ	kflʲoːkŋ̍	'fly'
salʲtsn̩	salʲts	ksalʲtsn̩	'salt'
ʃaɪsn̩	ʃaɪs	kʃɪsn̩	'shit'
ʃlʲɔːfm̩	ʃlʲɔːf	kʃlʲɔːfm̩	'sleep'
ʃmaɪsn̩	ʃmaɪs	kʃmɪsn̩	'throw'
ʃnaɪtn̩	ʃnaɪtʰ	kʃnɪt(ʰ)n̩	'cut'
ʃraɪpm̩	ʃraɪpʰ	kʃriːpm̩	'write'
ʃvaɪkŋ̍	ʃvaɪkʰ	kʃviːkŋ̍	'be silent'
ʃpalʲt(ʰ)n̩	ʃpalʲtʰ	kʃpalʲt(ʰ)n̩	'split'
ʃplʲaɪsn̩	ʃplʲaɪs	kʃplʲɪsn̩	'splice'
ʃprɛhʲɲ̍	ʃprɪç	kʃprɔhɴ̩	'talk'
ʃtɪŋk(ʰ)ŋ̍	ʃtɪŋkʰ	kʃtʊŋk(ʰ)ŋ̍	'stink'
ʃtraɪtʰn̩	ʃtraɪtʰ	kʃtrɪt(ʰ)n̩	'fight'
ʔɛndərn̩	ʔɛndər	kʔɛndərtʰ	'change'
raːtʰn̩	raːtʰ	kraːtʰn̩	'guess'
lʲyːkŋ̍	lʲyːkʰ	klʲoːkŋ̍	'(tell a) lie'
mɛsn̩	mɪs	kmɛsn̩	'measure'
neːmm̩	nɪm	knɔmm̩	'take'
vaksn̩	vaks	kvaksn̩	'grow'
hɔfm̩	hɔf	kʰɔftʰ	'hope'

b. No past participle prefix

Infinitive	*1sg present*	*Past Participle*	*Gloss*
paɩsn̩	paɩs	pɩsn̩	'bite'
prɛhʲɲ̩	prɩç	prɔhɴ̩	'break'
plʲaɩpm̩	plʲaɩpʰ	plʲiːpm̩	'stay'
tɛŋk⁽ʰ⁾ŋ̩	tɛnkʰ	taxtʰ	'think'
traːkŋ̩	traːkʰ	traːkŋ̩	'carry'
keːpm̩	kɩpʰ	keːpm̩	'give'
kriːhʲɲ̩	kriːç	krɔhɴ̩	'crawl'
klʲɔːpm̩	klʲɔpʰ	klʲɔpʰtʰ	'believe'
knaɩfm̩	knaɩf	knɩfm̩	'pinch'
p͡faɩfm̩	p͡faɩf	p͡fɩfm̩	'whistle'
p͡friːml̩ʲɲ	p͡friːml̩ʲ	p͡friːml̩ʲt	'cram'
p͡flʲeːkŋ̩	p͡flʲeːkʰ	p͡flʲeːkʰtʰ	'care'
kvɛlʲn̩	kvɩlʲ	kvɔlʲɲ̩	'swell *(intr.)*'
pʰak⁽ʰ⁾ŋ̩	pʰakʰ	pʰakʰtʰ	'be able to bear'
tʰɛʃt⁽ʰ⁾n̩	tʰɛʃtʰ	tʰɛʃt⁽ʰ⁾ətʰ	'test'
kʰɔfm̩	kʰɔf	kʰɔftʰ	'buy'
tsiːn	tsiː	tsoːkŋ̩	'pull'
tsvɩŋŋ̩	tsvɩŋ	tsvʊŋŋ̩	'force'

Apparently, only some stems add the past participle ('PP') prefix.[24] Comparing the data in (16a.) and (16b.), we find that the choice whether to affix the PP prefix is made according to the following constraint: If the stem-initial onset dominates a stop (other than ʔ or a nasal stop), do not affix a prefix; if it dominates a fricative, approximant, nasal stop or ʔ, select the prefix k-.[25] That is to say that, if we do not

[24]k when preceding r l v m n sound somewhat less strident than elsewhere; kʔ sounds like an ejective kʔ; I use 'kʔ' for IPA k' because I employ ''' for Korean tensed obstruents (*cf.* (8), p. 198). For those who would like to account for the apparent natural class r l v m n by assigning it the phonetically motivated feature [+voice] (mainstream) or the (supposedly) cognitively motivated element L/N (Nasukawa [336]), let me point out that this feature/element can to my knowledge not be successfully used to predict as well as GP can what constitutes a well-formed domain-inital, -medial or -final consonant cluster in a language. So if French voiced stops are assigned an L-head because of which they are well-formed constituent governors (brɛf 'short *(m.)*', drɛsaːʒ 'training', gratẽ 'casserole'), why are nasal stops and Nasukawa's other 'voiced' consonants (r l) bad constituent governors in all languages with branching onsets? Note that 'voiced' v is a good constituent governor in French (vrɛ 'true') but is, as equally 'voiced' consonant, an ill-formed constituent governor in (High) German or English.

[25]I neglect the differentiation between not adding a prefix and adding an empty alternant of the PP prefix.

take into account those PEs which do occur in AS but do not in Kikuyu, we find k- in AS where Kikuyu has ko-, and no prefix where Kikuyu displays ɣo-. Neglecting that the Kikuyu prefix contains a filled nucleus while in AS the nucleus licensing the onset of the prefix is empty and p-licensed, the following picture arises:

(17) *Dahl's Law in Kikuyu and Augsburg Swabian (neglecting nuclei)*

	Prefixal onset in *Kikuyu*	*Augsburg Swabian*	*Stem-initial onset*
a.	k	k	approximant, nasal stop (fricative, ʔ)
b.	ɣ	—	stop (other than a nasal stop or ʔ)

For an explanation of this phenomenon, we have to define one of the groups of stem-initial onsets as defined in (17a.) and (17b.) as natural class. We cannot use the presence of a ʔ-element as marker of the natural class in (17b.) because prenasalisation in many languages, *e.g.* Pulaar, Wolof, Bemba, Swahili, Gitonga[26] shows that nasal stops—which are in the class displayed by (17a.)—contain a ʔ-element. The following data from Pulaar exhibiting hardening and prenasalisation will illustrate this point:[27]

(18) *Pulaar hardening and prenasalisation*

Stem		*Hardening*		*Prenasalisation*	
a. hardening and prenasalisation observable					
waare	'beard'	bahel	'sm. beard'	mbahon	'sm. beards'
rawaandu	'dog'	dawaŋgel	'sm. dog'	ndawakon	'sm. dogs'
jahre	'scorpion'	ʤahe	'scorpions'	ɲʤahon	'sm. scorpions'
ʔamre	'turtle'	gamel	'sm. turtle'	ŋgamon	'sm. turtles'
b. hardening but no prenasalisation observable					
faaɓru	'frog'	paaɓel	'sm. frog'	paaɓon	'sm. frogs'
sawru	'stick'	ʧawel	'sm. stick'	ʧawon	'sm. sticks'
hoore	'head'	kojel	'sm. head'	kojon	'sm. heads'

[26]Regarding references for these languages, *cf.* Sylla [470] (Pulaar); Njie [341] (Wolof); Kula & Marten [279] (Bemba); Halle & Clements [197, p. 133], Miehle & Möhlig [325], Polomé [389], Schadeberg [432] (Swahili); Lanham [285] (Gitonga).

[27]There is a PhD thesis [341] (later published as [342]) on Wolof (Gambia) grammar displaying similar facts in relation to hardening and prenasalisation; unfortunately, Njie does not provide examples with domain-initial nasals which would have been relevant at this point of our discussion [341, pp. 38ff.] [342, pp. 39ff.]. I have IPA-ed Sylla's [470] y j into j ʤ (respectively); Sylla's symbol for the palatal implosive—which I do not have the relevant font for—has been substituted by IPA ʄ. Also, I have translated Sylla's French glosses into English. 'sm.' stands for 'small'.

c. no hardening and no prenasalisation observable

teppere	'heel'	teppel	'sm. heel'	teppoɲ	'sm. heels'
meselal	'needle'	meselle	'needles'	meseloɲ	'sm. needles'
ɓanndu	'body'	ɓalli	'bodies'	ɓalel	'sm. bodies'
ɗemŋgal	'tongue'	ɗemɗe	'tongues'	ɗemŋgel	'sm. tongues'
ʄamal	'engagement'	ʄame	'engagements'	ʄamoɲ	'sm. eng.ments'

We can explain the hardening phenomenon by saying that the forms in (18) displaying hardening (middle column) are formed on the basis of the stems in the leftmost column by fusing a ʔ-element with the melodic material dominated by the stem-initial onset point. We see that w (U) plus ʔ becomes b (ʔ · U), r (A) becomes d (ʔ · A), j (I) turns into ʤ (ʔ · I); in the same fashion, f (U · H), s (A · I · H), h (H) change into p (ʔ · U · H̲), ʧ (ʔ · A · I · H̲), h (ʔ · H̲) (respectively).[28] Those stem-initial PEs which already contain a ʔ-element in their underlying form, *e.g.* t m ɓ ɗ ʄ are not affected. Note that this list includes m, a nasal stop. This means that Pulaar hardening provides evidence for the claim that nasal stops contain a ʔ-element.

I have added the prenasalisation facts here because they illustrate that the fusion of an L-head (motivating voicing) goes hand in hand with the simultaneous fusion of a ʔ. For example, assuming that the prenasalised forms do not take the hardened but the non-hardened forms as their bases, L *and* ʔ are fused with w (U) resulting in b (ʔ · U · L̲). If we say that f (H · U), s (H · A · I), h (H), t (ʔ · A · H̲) are not affected by prenasalisation because they contain H and that, as discussed in section 5.2.3, L and H are incompatible in Pulaar, we see that m (ʔ · U · L) contains L and ʔ: m contains a ʔ because it is not affected by hardening, it contains L because it is not changed in its prenasalised version. Note that we can make this deduction independently of whether the prenasalised forms are built on the basis of the the hardened or the stem forms. Consequently we have evidence independent of such morphological questions which suggests that nasal stops contain L and ʔ. It follows that in the prenasalised forms (ʔ · L) is fused with the stem-initial onset; in the hardened forms, only ʔ is added.

Most importantly, this analysis of Pulaar initial hardening and prenasalisation points to the correctness of the claim made by Element Theory (and the RET) that nasal stops contain a ʔ-element. In relation to our discussion of Dahl's Law in AS this means that we cannot define the natural class triggering the loss of the past participle k-prefix as the PEs containing a ʔ. Since nasal stops, namely m (mɛs-) and

[28]Note that in line with my introduction to the RET in relation to consonants in section 4.1.3, ʔ and H only co-occur when H is head.

n (neːm-), *do* exhibit such a prefix (kmɛsn̩, knɔmm̩) while other stops do not (*e.g.* pɑɪsn̩ next to pɪsn̩), something else must define the set of domain-initial onsets in (16b.) as natural class. Note also that it is not possible to find something that all stem-inital onsets in (16a.) share. Therefore I claim that, as in Kikuyu, the underlying form of the prefixal onset-PE is k (ʔ · H̲); the H-head contained in this PE switches its H/O role (to operator) when the stem-initial onset (AS) or the leftmost filled stem-onset (Kikuyu) contains an H-head.

Interestingly, the AS facts help us to decide whether two consecutive H-elements (adjacent at the onset projection) or only two consecutive H-*heads* trigger Dahl's Law: Constituent governors, *e.g.* heads of branching onsets must be headed; in general, if such a governor contains H, H is the head. For example, p (ʔ · U · H̲) with an H-head is a well-formed constituent governor (p̲r̲ɛhʲ); since f functions as constituent governor too, *e.g.* in f̲r̲ɛs or f̲l̲ʲiːk, it must contain an H-head; f (U · H̲). s, on the other hand, can only contain H as operator but not as head; the reason for this is that s is an ill-formed constituent governor.[29]

This means that if we continue to assume that AS f contains an H-head—not an H-operator—because it is a good constituent governor, we cannot simply define the natural class triggering Dahl's Law in AS as the set of PEs containing an H-element or an H-head. If any stem-initial H-element were the relevant class marker, both f and s should trigger dissimilation; if only H-heads were to be considered the class-defining property, f should trigger dissimilation. We would have to propose that Dahl's Law is triggered when two consecutive onsets—the first of which belongs to the past participle (AS) or the infinitive (Kikuyu) prefix—both contain (ʔ · H̲), resulting in the H-element dominated by the left onset switching its H/O role. Note that as in the case of Pulaar prenasalisation, we would find that one of the tonal elements (here H; in the case of prenasalisation, L) is only phonologically active when fused with ʔ.[30] This gives us the following AS consonants containing an H-head: p (ʔ · U · H̲), p^h (ʔ · U)(H̲), t (ʔ · A · H̲), t^h (ʔ · A)(H̲), k (ʔ · H̲), k^h (ʔ)(H̲), f (U · H̲), s (U · H), h (H), and only those containing a ʔ-element (p p^h t t^h k k^h) trigger Dahl's Law.[31]

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[29]For arguments against an analysis of sC-clusters as branching onsets but an coda (s)-onset (C) sequences, *cf.* Kaye [256] and above, section 1.5.1.1 (pp. 42ff.).

[30]This would suggest that the glottal element needs to be substituted by a set of assumptions which displays fewer loose ends and requires fewer auxiliary hypotheses than necessary at present. This topic can, however, not be dealt with within this thesis.

[31]I neglect the two affricates p͡f and t͡s here. It suffices to say that Dahl's Law provides evidence for the claim that both affricates must contain the marker which triggers Dahl's Law-induced dissimila-

Note however that it might not be necessary to claim in agreement with the RET that AS f contains an H-*head* simply because it is a good constitutuent governor. In contrast to the RET, we could say that both f and s contain an H-operator, and that the reason why f is a well-formed but s an ill-formed constituent governor has to do with a restriction on s. The restriction I have in mind is to propose that languages can choose to disallow PEs containing an I-element from becoming constituent governors. In English, for example, ʧ, ʃ, ʤ and ʒ are bad constituent governors, so there are no branching onsets with *ʧr, *ʧl, *ʃr, *ʃl, *etc.*, and this state of affairs is not predicted by the RET. I therefore want to propose a constraint which excludes PEs containing I from (governing) onset heads:[32]

(19) *Barring* I *from onset constituent heads*

Onset constituent heads may not contain I (parameterised; default: OFF).

This way, ʧ, ʃ, *etc.*, are excluded from being dominated by the head of a branching onset but not from onset heads which transconstituently govern a preceding post-nuclear rhymal position ('coda'); ⟨belch⟩, ⟨march⟩ (for rhotic speakers), ⟨flinch⟩, ⟨cringe⟩ [ɲʤ] are well-formed in English. Interestingly, s may not be a constituent governor; that is to say that sC-clusters, *e.g.* in ⟨string⟩ or ⟨snow⟩, are not branching onsets but rhymal-onset sequences.[33] This fact on its own would not provide evidence in favour of the claim that s contains an I-element since there are other PEs which are bad constituent governors and which do *not* contain I, *e.g.* n (ʔ · U · L). However, there is cross-linguistic evidence which suggests that s contains I. For example, in Yimas (New Guinea, Foley [167]), words may not begin with or end in a palatal consonant; one of these consonants is c [*ibid.*, p. 39]; if not following a consonant, c is often realised as [s] [*ibid.*]. Apparently, s behaves like one of the palatal consonants, *i.e.* one of the PEs containing I.

Alternatively, consider the following situation in Indonesian (*cf.* Sneddon [452]): N in the prefix meN- symbolises a nasal consonant which is either lost or added ac-

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tion.

[32]This constraint might well be universal—under which languages would not 'choose' to have this restriction but would have to take it into account provided they allow onsets to branch. There is however no space here to discuss this matter in more detail. How closely linked this constraint is to the restriction which precludes I-heads (but not operators) linked to a nuclear position from being linked to a previous onset in Québec French is also beyond the scope of this chapter. In QF, a coronal stop is palatalised into an affricate when preceding i (I̲) or y (I · U̲) but not when preceding e (A · I̲) or ø (A · I · U̲). For data, *cf.* Kaye [253, pp. 29ff.], for an analysis for the QF vowel system, *cf.* chapter 6 below or Ploch [380, 384].

[33]For arguments in favour of this analysis, *cf.* Kaye [256].

cording to certain rules [*ibid.*, pp. 9ff.]; in the case of stem-initial s (*e.g.* in ⟨sewa⟩ seva 'rent, fare'), N plus s results in ɲ (ʔ · I · L) (⟨menyewa⟩ mɨɲeva 'to hire, rent'). Based on this evidence, we can say that s is not a good constituent governor in languages with branching onsets not because it—unlike f—contains no H-head but merely an H-operator but because *like* f contains an H-operator. This way, we can propose that not only PEs containing an H-head are good constituent governors but all PEs containing H—if no other constraint (like the one in (19)) prevents this. As a result, we get the following slightly altered version of the RET, which I will call Neo-RET. The only differences between the RET and the Neo-RET in (20) are firstly that a RET f contains an H-head while it contains an H-operator in the Neo-RET and secondly that a Neo-RET s contains I (universally) while the RET only assumed this to be the case in some languages:[34]

(20) *Heads of branching onsets with or without* H-*head in the Neo-RET*

Symbol	*RET*	*Neo-Ret*
p', p^h	(ʔ · U · $\underline{\text{H}}$), (ʔ · U)($\underline{\text{H}}$)	(ʔ · U · $\underline{\text{H}}$), (ʔ · U)($\underline{\text{H}}$)
t', t^h	(ʔ · A · $\underline{\text{H}}$), (ʔ · A)($\underline{\text{H}}$)	(ʔ · A · $\underline{\text{H}}$), (ʔ · A)($\underline{\text{H}}$)
k', k^h	(ʔ · $\underline{\text{H}}$), (ʔ)($\underline{\text{H}}$)	(ʔ · $\underline{\text{H}}$), (ʔ)($\underline{\text{H}}$)
f	(U · $\underline{\text{H}}$)	(U · H)
s	(A · H)	(A · I · H)

This small change to the ToE enables us to define the context triggering Dahl's Law more neatly. We can now view Dahl's Law as a case of dissimilation which avoids two onset adjacent at the onset projection if the first of them dominates a PE (k) containing an H-head (being a member of a particular prefix) and the second dominates a PE containing an H-head too. Given a certain context, Dahl's Law (in Kikuyu and AS) is simply H-head dissimilation via H/O role switching in the onset licensee. It is not necessary in such an analysis to refer to the ʔ-element in any way.

Note also that the term 'dissimilation' does not explain much. Interestingly, in all known cases of Dahl's Law it is the left onset which is 'dissimilated'. As in Kikuyu, we can explain the dissimilation attested in AS as a case of right-to-left inter-onset government, with the prefixal onset being licensed by the stem-initial onset.[35] I have already discussed the Korean case of onset-to-onset licensing where PEs containing

[34]In line with the RET, I continue to assign s an A-element (motivating coronality for consonants). In languages which have s but do not differentiate s from ʃ and in which there is no evidence for the assumption that s contains A, s might well not contain A. But this is another story.

[35]In Kikuyu, not the stem-initial but leftmost filled onset of the stem is taken into account.

H may not be inter-onset licensed (*cf.* above and chapter 4). The restriction describable as Dahl's Law is similar to this: In cases of Dahl's Law, H-*heads* cannot be licensed; H-heads which are not licensed are delinked (because switch their H/O role would result in the univerally excluded co-occurrence of ʔ and an H-operator).

Let me add that, in order for this analysis to work, we have to take into account that there are two types of stops in AS: non-aspirated stops (which are acoustically somewhere between tense and neutral), *e.g.* p, and aspirated voiceless stops, *e.g.* p^h. Since both types trigger Dahl's Law, both types must contain an H-head. Therefore I propose that AS like Korean exhibits both types of theoretically possible H-headed stops: those with fused (p/p' (ʔ · U · $\underline{H}$)) and those with extrapolated H-head (p^h (ʔ · U)($\underline{H}$)).

I conclude that Dahl's Law is not as Nasukawa [336] seems to think motivated by voicing dissimilation or a 'cognitive' version of such an analysis but by the restriction on inter-onset licensing relationships that not both (right-hand) licensor and (left-hand) licensee may contain an H-head. In section 5.2.4.2, I will show that there is a very similar constraint on L-heads dominated by the (left) inter-onset licensee.

5.2.4.2 Meinhof's Law

In section 5.2.4.1, I have provided evidence for my claim that Dahl's Law is a manifestation of onset-to-onset government. I have shown that there is independent evidence from Korean (which does not exhibit Dahl's Law) that the licensee-properties of H are limited and that this limitation might be exploited by a language. This analysis of Dahl's Law will, in turn, support the claim that I would like to make in this section. This claim is that there is an identical constraint regarding L-heads dominated by onset-to-onset licensees.

To introduce the relevant data, let me refer to Kula & Marten's [279] article on nasality in Bemba, a Zambian Bantu language.[36] As Kula & Marten point out, Schadeberg [431], Gerhardt [171] and Evans [154] provide further information about Meinhof's Law.

In order to be able to discuss Meinhof's Law in Bemba, I first have to talk about the first singular prefix N. In Bemba, this prefix is added to a stem according to the following restrictions: Firstly, N is homorganic to the stem-initial consonant. Secondly, stem-initial β l becomes b d after N, resulting in mb nd. Thirdly, if the

[36] Kula & Marten's Bemba sources are Hoch [219] and Sharman [443]; Kula herself is a native Bemba speaker.

stem starts with a vowel, *i.e.* if left-most stem onset is empty, N turns into ŋg.[37] To provide some examples, consider the following data (taken from Kula & Marten [279, pp. 192f.]):[38]

(21) *First singular* N-*prefix with Bemba verb stems*

a. Homorganic nasal

-pata	mpata	'hate'
-tana	ntana	'refuse'
-kula	ŋkula	'grow'
-ʧapa	nʧapa	'wash'
-futa	mfuta	'pay'
-sala	nsala	'choose'
-ʃita	nʃita	'buy'
-masa	mmasa	'daub'
-naka	nnaka	'get tired'

b. Homorganic nasal plus hardening of β l

-βila	mbila	'sew'
-leka	ndeka	'stop'

c. Vowel-initial stems

-ubula	ŋgubula	'peel'

To explain this pattern, let us say that the N-prefix consists of an onset-nucleus pair; N is attached to an onset, and this onset, like all onsets, is licensed by the nucleus it precedes. This prefix is attached to the left edge of a stem. Since N-prefixation creates sequences which are not possible when no morphology is involved (*e.g.* mf, mp), let us also propose that the N-prefix is an analytic affix;[39] in other words, the phonology function ϕ is applied to the N-prefix before affixation (*i.e.* before con-

[37] I neglect that, additionally, the leftmost stem onset, if it is lexically empty, is linked to a I-element that is dominated by the following nucleus. So N preceding vowel-initial stems have ŋg instead of N if the vowel in question is a, u, o, *i.e.* when the vowel does not contain I. If the left-most nucleus of a stem beginning with an empty onset dominates i or e—both i and e contain I—, I spreads to the initial onset, resulting in ɲʤ (n̠ʤ for Kula & Marten) (ʔ · I · L) (instead of ŋg) (ʔ · L).

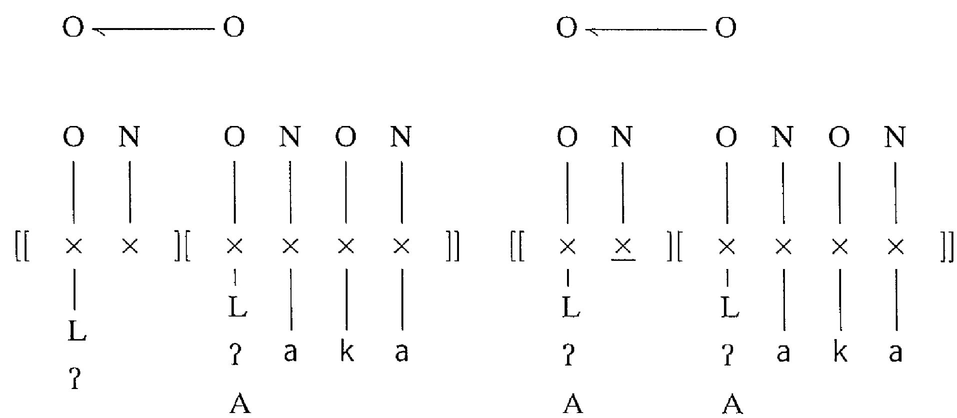
[38] Kula & Marten have the following lexical consonant inventory—I have added likely Neo-RET candidates: p (ʔ · U · $\underline{\text{H}}$), t (ʔ · A · $\underline{\text{H}}$), k (ʔ · $\underline{\text{H}}$), ʧ (ʔ · A · I · $\underline{\text{H}}$), f (U · H), s (A · I · H), ʃ (A · I · U · H), m (ʔ · U · L), n (ʔ · A · L), ŋ (ʔ · L), β (ʔ · U), l (ʔ · A).

[39] Regarding a cognitive view on morphology and the differentiation between 'analytic' *versus* 'non-analyitic' morphology, *cf.* Kaye [259].

catenation). Using the example of the verb stem naka, we get [[N∅][naka]]—*i.e.* $\phi[concat(\phi[\text{N}\emptyset], \phi[\text{naka}])]$—, which has the following constituent structure:[40]

(22) *Bemba* nnaka

a. *at LL* b. *at PL*



Starting with the forms in (21a.), let me propose that the aperture element(s) dominated by the stem-initial onset spread leftwards at the onset projection to the prefixal onset. So in (22b.), A spreads from stem-initial n (ʔ · A · L) in <u>n</u>aka to the prefixal onset—which lexically dominates (ʔ · L)—, resulting in derived n (ʔ · A · L) in the prefix. This patterns appears to work for all the stems in (21). However, we have not yet accounted for the postnasal hardening effects in (21b.–c.), *i.e.* in stems beginning with an empty onset or an onset dominating β or l. To do so, we have to ask ourselves what natural classes are involved. Assuming that N contains a ʔ-element—as nasal stops do (ʔ · L))—we could propose that β l contain no glottal and that both L and ʔ spread, independently of each other, rightwards, turning β (U) and l (A) into mb (ʔ · U · <u>L</u>) and nd (ʔ · A · <u>L</u>) (respectively). Even though l contains a ʔ-element in other languages, *e.g.* in Korean (*cf.* section 4.1.3), we could say that the acoustic difference between r and l is not relevant in Bemba because there is no phonological contrast; whether (A) is phonetically realised as [r] or [l] is mere phonetic 'packaging' (Kaye's [262, p. 210] term). The problem with this view is that stem-initial f s ʃ—which do not contain ʔ—do not get hardened by ʔ-addition.

For the purposes of the present discussion, it suffices to point out that in a more complete analysis of Bemba phonology it would have to be motivated phonologically, *i.e.* cognitively, not phonetically, whether β l contain a ʔ-element in Bemba or not. For example, it would not be an argument to propose that β could not contain ʔ because β is not a stop. In the following, I will assume for argument's sake that β l

[40]Each nucleus is dominated by a rhyme, which is not given in (22).

contain ʔ.

At this point of the discussion, we still have not explained why f s ʃ are not fused with ʔ or L or why p t k ʧ, which already contain ʔ as part of their lexical representation, are not fused with L. Let me therefore propose three more competing analyses. The first one consists of the assumptions that L (as part of the N-prefix) is actually fused with the stem-initial onset in (21b.–c.), and that ʔ-fusion is parasitic on L-fusion; so ʔ only moves alongside L. That is to say that all of the melodic make-up of the onset of the N-prefix has to be fused with the stem-initial onset or none of it. Assuming that L fuses as head (independently of its lexically assigned operator role in N), N-β (ʔ · L)-(ʔ · U) turns into mb (ʔ · U · L̲)—resulting in one stem-initial PE, with no melodic material attached to the prefixal onset point at PL. In the case of an empty stem-initial onset (21c.), L is attached, and parasitically on that, ʔ, and we get derived ŋg (ʔ · L̲). The reason why L is not attached to any of the stem-initial onsets displayed in (21a.) is that they either already contain L (m n), or they contain an H-element (p t k ʧ f s ʃ). I have already shown in in section 5.2.3 that, in many languages—including many Bantu languages—L and H do not fuse, *i.e.* are 'incompatible'. Now it is also clear why f s ʃ are not hardened, *i.e.* why ʔ is not added: because L is not fused with the stem-initial onset and ʔ-fusion is parasitic on L-fusion. To sum up our first analysis, L, and parasitically on that, ʔ, move from the prefixal onset and are fused with the stem-initial onset. If as in the case of the stems in (21a.) fusion is not possible, L and ʔ remain in their lexically specified onset.

Alternatively, we could agree with analysis 1 in all points but one: we could claim that the N-prefix is not attached to its own constituent or point, but is motivated by a floating nasal, *i.e.* by floating (ʔ · L). In this scenario there would only be one relevant onset, the stem-initial onset, and no p-licensed empty nucleus. Floating N would try to fuse with the PEs dominated by the stem-initial onset, and if fusion is ungrammatical because the target already contains L or contains H, N would attach itself to the onset in form of a light diphthong, *i.e.* as separate PE, without fusion. Since there is a universal ordering between N and a stem-initial PE it cannot fuse with, that is to say, since the prefixation of N to a stem beginning with an onset dominating H, *e.g.* p (ʔ · U · H̲), never results in a situation where N follows the voiceless obstruent (*pm), we can claim that this asymmetry is due to a head-final licensing relationship with N being licensed by the following stem-initial onset.[41] Furthermore, the light

[41] I neglect a question which can be asked about any light diphthongs, *i.e.* independently of my analysis here: Why do light diphthongs (consisting of two in principle unordered PEs) display a typical inherent order in their acoustic realisation. So if we say that mp in mpata is a light diphthong

diphthong analysis would ignore that the N-prefix is an analytic morph: Since N comes with its own domain, this domain must contain at least one onset-nucleus pair. It seems analysis 1 (analytic N with L-movement) is more empirical than analysis 2 (floating N creating light diphthongs).

The third analysis I would like to present here is as follows: If we claimed that, as in analysis 1, N is linked to its own onset and that in the forms in (21b.–c.) L and ʔ do not move (as in analysis 1) but copy themselves (*i.e.* spread) into the stem-initial onset, we would also predict the correct result, *i.e.* that the lexical onset-onset sequence N-β, *i.e.* (ʔ · L)-(ʔ · U)—, which are separated by a p-licensed (silent) empty nucleus but are adjacent at the onset projection—would turn into derived m-b (ʔ · U · L)-(ʔ · U · $\underline{\text{L}}$).[42] As in analysis 1 and 2, we also assume that L is fused as head independently of its lexical H/O in N. In this analysis, both prefixal and stem-initial onset dominate melodic material at LL and PL. It seems that based on the data in (21), it cannot be decided which analysis, 1 (L-movement) or 3 (L-spreading) is to be preferred.

It is my unified account of Dahl's Law (*e.g.* in Kikuyu and AS) and Meinhof's Law (*e.g.* in Bemba) which will help us to decide which of the two remaining analyses of Bemba N-prefixation presented above is the more empirical one. Consider the following data (taken from Kula & Marten [279, p. 193]):

(23) *Meinhof's Law in Bemba*

a.	-βomb-	mombele	*mbombele	'I have worked'
b.	-land-	nandile	*ndandile	'I have spoken'
c.	-ond-	ŋondele	*ŋgondele	'I have become thin'
d.	-paŋg-	*maŋgile	mpaŋgile	'I have made'
e.	-βeleŋg-	*meleŋgele	mbeleŋgele	'I have read'

We see in (23a.–c.) that we do not get the expected initial prenasalised voiced stops if the following consonant is also a prenasalised (voiced) stop. We find the corresponding homorganic nasal stops instead. This dissimilation is referred to as 'Meinhof's Law'. Note that in such cases, stem-internal prenasalised voiced stops must be voiced since Bemba does not allow prenasalised voiceless stops. (23d.) illustrates that initial prenasalised voiceless stops (created by N-prefixation) do not trigger Meinhof's Law.

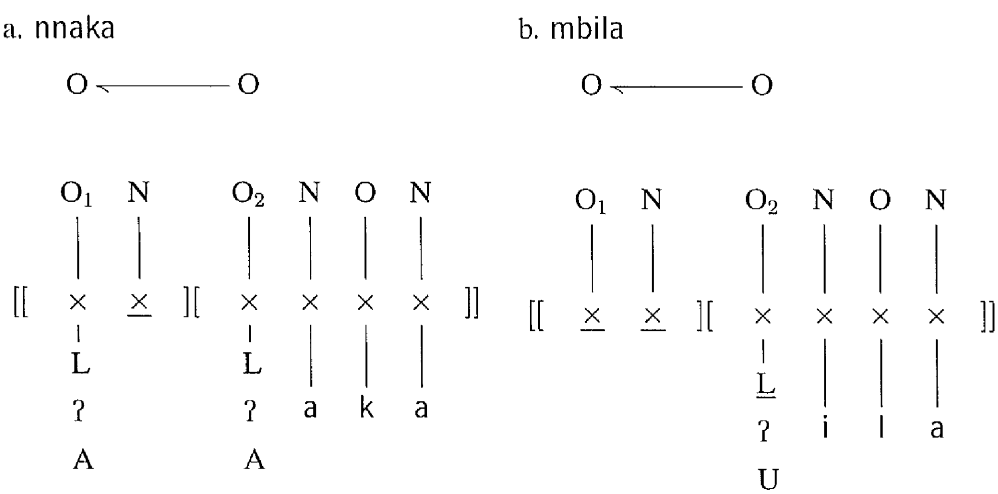
attached to one and the same onset point, why is this light diphthong never pronounced [pm]? Or maybe it is somewhere. I have to leave this topic to future research.

[42]I assume that in analysis 3, b in mb is motivated by (ʔ · U · $\underline{\text{L}}$), not by neutral b/p (ʔ · U) because, universally, neutral stops are bad governors.

The example in (23e.) shows that the there may be no consonant intervening between two prenasalised voiced stops if Meinhof's Law is to be applied.

We cannot explain this dissimilation phenomenon as part of a unified account for Dahl's and Meinhof's Law, if analysis 3 is correct. In analysis 3 (two onsets with intervening empty nucleus and L-spreading), prenasalised stops are seen as structurally complex segments— which are unlike the PEs triggering Dahl's Law. Let us therefore propose that analysis 1 is correct: N-prefixation operates as represented in (22). However, the picture presented there is not complete. In the example used, we see two separate PEs n n since stem-initial n (ʔ · A · L) already contains L. All examples in (21a.) would be identical in this respect, *i.e.* prefixal N and the PE dominated by the stem-initial onset would be linked to two separate onsets. In the examples in (21b.–c.), on the other hand, *i.e.* when fusion takes place, all elements belonging to the N-prefix (L and ʔ) move to the stem, the (now empty) structure of the prefix is retained though. Even though there is only little known about the p-licensing of onsets—other than an empty onset point in French (*h aspiré*) being properly governed by its nucleus (*cf.* Charette [83, pp. 88ff.])—, let me propose that the empty onset point, which, according to the Empty Category Principle (*cf.* Kaye [255] and section 3.1.4.2), has to be p-licensed as empty category in order to be silent, is p-licensed via inter-onset government:[43]

(24) *Bemba* nnaka *and* mbila *at PL*



[43]If we want to say that both L and ʔ either fuse with the stem or are both dropped, it is necessary to claim that β l contain no ʔ. Otherwise, we would predict that L, but not ʔ, would spread into β l, resulting in two onsets the first of which would dominate ʔ, the second (ʔ · U · L) or (ʔ · U · L) (for mb or nd respectively). Again, this structurally complex version of mb and the dissimilation stated as Meinhof's Law could not be explained via an account similar to the one I have proposed for Dahl's Law.

Note that in this analysis, in both (24a.) and (24b.), O_2 inter-onset licenses O_1; this is why the empty prefix is retained in (24b.). This means that I am assuming some kind of Empty Syllable Constraint, *i.e.* a universal restriction which disallows the generation of unlicensed onset-nucleus pairs which dominate no melodic material. Let me propose that an onset-nucleus pair is always licensed in the lexicon; it may be licensed melodically, *i.e.* by dominating melodic material, or by inter-constituent government. Remember that onsets and nuclei always come in pairs in GP (amongst other theories), which is predicted by a combination of two claims—both of which have been proposed independently of each other: Kaye's [257] "A nucleus can and must license an onset to its left", and Harris's 'Onset Licensing Principle' ("an onset head position must be licensed by a nuclear position", [199, p. 380]). As pointed out in chapter 4, neither of these principles is able to predict what it was assumed for, *i.e.* that onsets and nuclei occur in pairs ($[\mathrm{ON}]^n$).[44] Let me therefore propose the ON-Licensing Principle:[45]

(25) *The ON-Licensing Principle (ONLP)*

1. A nucleus must license an onset to its left.
2. An onset must be licensed by the nucleus to its right.

With this theoretical background, I can now propose (my version of) the Empty Syllable Constraint:

(26) *The Empty Syllable Constraint (ESC)*

A p-licensed empty nucleus cannot properly-govern its onset.

This universal constraint ensures that an empty syllable is only grammatical if its onset is p-licensed by some licenser other than its nucleus (*i.e.* other than the nucleus immediately following the (empty) onset of that empty syllable), which is only possible if the onset is licensed via inter-onset government. In relation to Dahl's and Meinhof's Law, it can be said that in line with the ECS the nucleus of an empty syllable is p-licensed via inter-onset government. A corollary of this is that, when empty syllables are involved, an inter-onset licensing relationship p-licenses both the (preceding) onset—which is the onset of the empty syllable—and the nucleus intervening between the two onsets in question—*i.e.* the nucleus of the empty syllable.

[44] '$[\mathrm{ON}]^n$' is virtually identical to Vergnaud's [483] '(O R)*' (*cf.* KLV [267, pp. 200f.]).

[45] I eliminated Harris's specification for head positions. Since there are ON-pairs with pointless (*i.e.* positionless) onsets (*cf.* Charette [83]), his principle undergenerates.

An earlier (somewhat less precise) version of this proposal of licensed empty syllables was given by Lowenstamm [307] who proposed that the cross-linguistically possible observation that languages allow more consonantal constrasts domain-initially than -medially or -finally can be explained by the proposal of a new phonological site, *i.e.* an empty onset-nucleus pair (CV pair for Lowenstamm) at the beginning of a domain; this empty pair is subject to certain licensing phenomena.

Importantly, the onset in the AS PP prefix in the forms in (16b.) (silent prefix) is inter-onset licensed by the stem-initial onset because of which the melodically empty version of this prefix is well-formed. Similarly, in (24b.), the stem-initial onset licenses the non-empty onset of the nasal prefix; in this case, however, there is no empty syllable, and the conditions for the application of the ESC are not met.

Having found out on the basis of an analysis of Meinhof's Law that there are empty inter-onset licensed prefixes, we can apply this to our account of Dahl's Law in AS, where, in certain circumstances, there is an empty prefix. In AS, this occurs when Dahl's Law is triggered and all melodic material of the prefix is delinked. In such cases, there is not no prefix at PL but an empty one.

Based on analysis 1 (attempted L-movement and -fusion), we are now able to account for Meinhof's Law. All we have to say is that, as in the case of Dahl's Law but here with L instead of H, a PE containing an L-head cannot be licensed by a PE which also contains an L-head. If due to prefixation, *i.e.* attempted fusion, a prenasalised voiced stops (a PE containing an L-head) is created, the following onset may not also dominate a PE displaying an L-head. As in Dahl's Law, the licensing clash is avoided by switching of the relevant element (here L) from (lexical) head to (derived) operator role. One difference between Dahl's Law in Kikuyu and AS, on the one hand, and Meinhof's Law in Bemba is that Dahl's Law applies between two onsets the first (left) of which belongs to a specific prefix and the second of which is the stem-initial onset; Meinhof's Law, however, takes into account the stem-initial onset and the following (stem-internal) onset. Another difference has to do with the universal ungrammaticality of fusion of a ʔ-element with an H-operator; in other words, in order for ʔ and H to fuse, H must be head. There is however no such constraint on L; so both (ʔ · L) and (ʔ · $\underline{\text{L}}$) are fine. This way the result of the attempt at avoiding an H-head licensing clash, *i.e.* (ʔ · H) is ill-formed, resulting in (ʔ) which is ungrammatical in AS. In the case of Meinhof's Law H/O role switching of a lexically assigned L-head in the stem-initial nasalised onset to an operator role does not result in an ill-formed expression: So mb (ʔ · U · $\underline{\text{L}}$), nd (ʔ · A · $\underline{\text{L}}$), ŋg (ʔ · $\underline{\text{L}}$)

comes out as m (ʔ · U · L), n (ʔ · A · L), ŋ (ʔ · L) (respectively).

Finally, note that Meinhof's Law can only be triggered between two onsets adjacent at the onset projection both of which would, without the application of Meinhof's Law, contain an L-head. Consequently, in the case of mpaŋgile (*maŋgile, (23d.)), where m and p belong to separate onsets and are unfused (*cf.* nnaka in (24)), there is no L-head involved: m in mp is cognitively motivated by (ʔ · U · L), p by (ʔ · U · $\underline{\mathrm{H}}$). Since the conditions triggering Meinhof's Law are not met, L-head dissimilation is not observable.

Importantly, if we want to accept this analysis we have to accept too that the inter-onset licensing relationship resulting in the phenomenon labelled 'Meinhof's Law' is *not* dependent on complexity, even though other inter-onset licensing relations are. For example, in AS, stem-initial r (A) can license prefixal k (ʔ · $\underline{\mathrm{H}}$). This is not problematic as long as we do not claim that Harrris's Complexity Condition is expressed via each and every licensing relationship. Note that the RET already claims that there are licensing relations which are not defined by complexity. So in languages with branching onsets, any PE containing H, *i.e.* f (U · H), is a well-formed constituent governor (as long as it does not contain I, *cf.* (19)). Similarly, in Korean (*cf.* section 5.2.4.1), complexity is only involved in the definition of the substantive constraints on inter-onset licensing if there is no H-head present in a PE; all Korean consonants exhibiting an H-head are well-formed inter-onset licensers. We see, it is not problematic to say that the inter-onset licensing relationship behind Meinhof's or Dahl's Law is independent of complexity but is solely motivated by the avoidence of identical heads in two consecutive onsets.

In (23e.), it is the restriction on Meinhof's Law that the two onsets involved have to be adjacent at the onset projection which is not observed; since there is another onset (*i.e.* the onset dominating (leftmost) l in mbel̲eŋgele) intervening between the two prenasalised voiced stops, the two L-headed expressions in this example, *i.e.* mb (ʔ · U · $\underline{\mathrm{L}}$) and ŋg (ʔ · $\underline{\mathrm{L}}$), are not adjacent.

To sum up, I have provided a unified account for Dahl's and Meinhof's Law in this section. It is an essential part of this analysis, that old N and old L are merged into one new element L which motivates, when linked to onsets or onset licensees, (prenasalised) voiced obstruents (as L-head) and nasal consonants (as L-operator). Because of this merger, a parallel between H/O role switching of H (Dahl's Law) and of L (Meinhof's Law) can be drawn. So in the case of Dahl's Law, an H-head must not be inter-onset licensed. Similarly, in Korean, any H-element cannot be inter-onset licensed. Let me add that, as mentioned above, there are restrictions on L-heads in licensed position in many languages, which also vary with respect to the precise nature of such a constraint. For example, in her analysis of German final obstruent devoicing, Brockhaus [66]—who still distinguishes L from N—finds that L^-, in terms of the (neo-)RET: an L-head, is unlicensed in onsets which are licensed by a domain-final p-licensed empty nucleus (⟨Rad⟩ raːt 'wheel', ⟨Rades⟩ raːdəs 'wheel's') or when dominated by a postnuclear rhymal position which is licensed by such an onset (⟨Smaragd⟩ smɑrɑkt 'emerald', ⟨Smaragdes⟩ smɑrɑkdəs 'emerald's'). Brockhaus also reports that there are some varieties of German which exhibit such a constraint word-medially (⟨regnen⟩ reːknən—reːgnən for others—'to rain').

All in all, it can be said that the main advantage of the proposed merger of L and N is that it links Dahl's and Meinhof's Law to different types of prosodic licensing attested cross-linguistically and that it, in line with Popper's requirements for the growth of knowledge (*cf.* Popper [392, pp. 240ff.]), connects hitherto unconnected facts (requirement 1, [p. 241]), and is independently testable by leading to "phenomena which have not so far been observed" (requirement 2, [*ibid.*]), in our case: the absence of a prefix in AS explained as H-head dissimilation. More importantly, as I have shown in my analysis of Turkic vowel harmony (chapter 4, [383]) and will support further by an account of N-prefixation in chapter 7, my version of the Acquisitional Hypothesis points to a phenomenon previously gone unnoticed: Due to acoustic cue overlap many of the consonant or vowel systems attested cross-linguistically can only be acquired with the help of phonotactic constraints or phonological processes disambiguating the overlap.

Future research will show whether my new explanation/theory stands up to Popper's third requirement, *i.e.* (continued) severe empirical testing of the new theory.[46]

Conclusion

I have shown in this chapter that facts from many different languages point to the fundamental correctness of the claim that old N and old L can be merged into one new element L. It has become clear that L-heads dominated by onsets motivate acoustic voicing and prenasalisation in obstruents; an L-operator attached to an onset is pho-

[46] I neglect here that, in my view, theories have no empirically relevant status; only hypotheses matter. A theory is simply a set of hypotheses which in its totality can, as a matter of fact, (virtually) always be falsified. Also, which hypotheses are actually selected for a particular theory is often a pragmatic decision, not an empirical one.

netically realised as consonantal nasality. The evidende supporting these proposals is based on analyses of postnasal voicing, hardening, prenasalisation, nasal licensing (nasal sharing) and the two dissimilation laws Dahl's Law and Meinhof's Law.

Chapter 6

Nasal Vowels

Introduction

I have already shown in chapter 4 that a cognitive view of phonological nasality avoids the circularity inherent in any phonetically grounded approach to nasality. Let me remind the reader that my study of NVs in Mahu, Lobiri and Jula, on the one hand, and in Québec French ('QF') and Montpelier French ('MF'), on the other, indicates clearly that there is evidence for four types of NVs: nasal monophthongs ('NMs')—motivating t2 NVs (*i.e.* exhibiting immovable N)—in Jula, Mahu and Lobiri; heavy nasal diphthongs ('NDs') in QF and MF—also with rigid nasality;[1] light NDs—triggering nasal spreading or movement—in Mahu and Lobiri, and vowels with floating N (t1 NVs in Jula).

Having merged (old) N and (old) L into (new) L, these points are still valid. We simply have to update the findings of chapter 4. So it is the distinction between floating L and fused L, and between light and heavy diphthongs containing L ('L-diphthongs') which we can refer to in order to explain the four types of NVs in question.

What I would like to add to this in this chapter is a discussion of NVs in QF and Portuguese. Both languages are interesting in the context of a cognitive account of

[1]When I say 'rigid' in relation to French NVs, I am referring to the fact that, unlike Jula, Mahu or Lobiri, French nasality does not nasalise following consonants or vowels. I do realise that there is a case to be made for French about domain-final floating N, *e.g.* in fin, fẽ 'nice (f., m.)' (*cf.* (18) in chapter 3, p. 146). The argument for this is not that there are morphologically related forms in French which exhibit domain-final alternations between NVs and Vn-sequences but that there are no examples where a domain-final Vn-cluster in the feminine forms of certain adjectives do not display a NV in the corresponding masculine forms; so there is no French adjective of the type fin~fe or fin~fi. In other words, given the feminine form of an adjective, if this form ends in a non-nasal consonant, this consonant may simply disappear in the masculine form (*e.g.* in vɛr 'green (m.)' next to vɛrt 'green (f.)'), if it ends in a nasal consonant, the masculine form must exhibit a NV.

phonological nasality in that they exhibit distributional gaps for NVs which cannot be observed for the corresponding series of OVs. More specifically, QF (like other languages commonly referred as 'varieties of French') does not have high NVs. I have shown in chapter 3 that universalists of synchronic as well as of diachronic flavour have tried to argue that such a gap—or preferred nasalisation on low vowels—can be motivated phonetically. However, I have made it clear that such proposals are utterly preposterous. In the same chapter, I have promised that I will provide a cognitive explanation of the apparent affinity between lowness and nasality. To this end, section 6.1 contains an analysis of QF NVs in which I will demonstrate that the (cognitively established) phonology forms a relevant part of an explanation of the apparent 'phonetic' gap, *i.e.* the absence of high NVs. This analysis will be backed up by a discussion of Montpelier French VN-clusters and English NC-sequences. In section 6.2, I will look at stressed vowels in Portuguese and provide a cognitive account of the apparent neutralisation of tense-lax distinctions in NVs.

My conclusion will be that a cognitive approach to the phonological nasality is more empirical than a phonetically motivated analysis.

6.1 A re-evaluation of the Heightmyth: nasal vowels in Québec French

6.1.1 The proposals

In all varieties of French that exhibit NVs, such NVs contain the element A. In this section,[2] I will try to explain some of the reasons why this should be. GP makes no claims about any special affinities between at least some of the elements in use. Considering that nasality is seen as triggered by the occurrence of L within a PE, the question arises: why then, if there is no affinity between A and L, do all French NVs have to contain A? I will make the following proposals:[3]

1. A QF NVs is a PE linked to a branching nucleus at the level of lexical representation ('LL'). This is why such a NV sounds phonetically long. In other words, it is the phonology which motivates the phonetics, not *vice versa*.

2. A QF NV contains an L-element which is not fused with the rest of melodic make-up of that NV but which is dominated by the onset following the branch-

[2]This section is a revised and updated version of parts of Ploch [380].

[3]I will not deal with liaison in this chapter. All QF and MF data is mine and is based on work with native speakers of these two languages.

ing nucleus.

3. In QF, the governor of a branching nucleus must dominate a PE containing A. I will try to show that this situation is independent of NVs, but has to do with properties of A. I will motivate this with facts taken from QF and Yawelmani.

4. It makes no (linguistic) sense to subcategorise a 'language' into smaller socially defined 'varieties' or 'dialects'. The fact that both QF and MF NVs are motivated by heavy NDs is as irrelevant to an analysis of MF NVs as it is to an explanation of QF NVs.

5. In QF, NVs contain an L-operator (linked to an onset position).

One could try to counter my objection to the proposal of some special A/L-affinity by pointing to the section on L/H-incompatibilty (5.2.3) which I referred to in order to explain depressor-induced H-dislocation in Zulu. Similarly, my account of Bemba N-prefixation presented in section 5.2.4.2 argues that voiceless fricatives (f s ʃ) are not hardened when following N because ʔ-fusion is parasitic on L-fusion which in turn is impossible in the case of stem-initial voiceless fricatives (and stops) due to L/H-incompatibility. Are inter-elemental affinity and incompatibility not two sides of the same coin? My reply to this is firstly that I will avoid any proposal of inter-elemental affinities or incompatibilities where possible, *i.e.* I will reject the proposal of L/A-affinity if I can, and secondly that there *is* an alternative explanation for A/L-affinity—which I will discuss in the following—while there is none for L/H-incompatibility.

6.1.2 The data

There are eight domain-final (stressed) oral vowels or, to be more precise, eight PEs not containing L, that can be linked to a nucleus. In line with Kaye [257, 262], I assume that, in QF, all such domain-final vowels are headed:

(1)

a	(A̲)	vjẽdra̲	⟨viendra⟩	'will come (3sg)'
i	(I̲)	ki̲	⟨qui⟩	'who'
u	(U̲)	nu̲	⟨nous⟩	'we'
y	(I · U̲)	tsy̲	⟨tu⟩	'you'
e	(A · I̲)	ale̲	⟨aller⟩	'go (inf)'
o	(A · U̲)	wazo̲	⟨oiseau⟩	'bird'
ɛ	(I · A̲)	lɛ̲	⟨lait⟩	'milk'
ø	(A · I · U̲)	fø̲	⟨feu⟩	'fire'

Charette [85] argues that QF has the GC 'U must be head' so that all PEs containing U as operator are ungrammatical in QF, in other words, precisely those four headed PEs which do not occur in QF. However, in line with Kaye [262], I will assume the GC 'Nothing can license U'.[4] I propose the following GCs for stressed QF vowels:

(2) *GCs for stressed vowels in Québec French*

1. PEs must be headed.
2. U must not be licensed.

Additionally, there are four NVs in QF, namely ã ẽ õ ø̃:

(3) *Québec French nasal vowels*

A	fused with L yields	ã	as in	dã	⟨dans⟩	'in'
A, I		ẽ		vẽ	⟨vin⟩	'wine'
A, U		õ		sõ	⟨sont⟩	'are (3pl)'
A, I, U		ø̃		brø̃	⟨brun⟩	'brown (m)'

Assuming L fuses with certain combination of A, I and U, the following questions arise:

1. Why do all of the QF NVs have to contain A? Or, why is it that there is no *ĩ, *ũ or *ỹ?

2. What is the internal representation of QF NVs?

[4] I will discuss this particular LC in more detail below. Since there is short ɔ (A · U), *i.e.* a headless expression, in QF, the GC 'U must be head' makes a wrong prediction. Note, however, that the GC 'U in a headed expression must be head' which I proposed in Ploch [380] cannot be backed up by independent evidence. In other words, no other language appears to exhibit a GC of the type 'Element X in a headed expression must be head'. I therefore agree with Kaye [262, p. 217] who proposes 'Nothing can license U'. Note that Kaye proposed this GC for Continental French; differences between Continental and Québec French do however not appear to be relevant to a discussion of the correct GC on U-element for stressed vowels in these varieties.

3. Why does QF exhibit precisely these four NVs?

4. Are QF NVs lexical or derived, *i.e.* is the 'nasal' element fused with other elements to form a NV at LL or is L added via phonological derivation?

6.1.3 Properties of Québec French nasal vowels

6.1.3.1 Proposal 1: QF NVs are headed

Comparing English and QF with respect to domain-final unlicensed (headed) nuclei that contain A (neglecting NVs), one observes an asymmetry: while both languages have the two vowels which have A as an operator (e (A · I̲) and o (A · U̲)), when A is head, in English only ɔ (U · A̲) is grammatical whereas in QF only ɛ (I · A̲) is grammatical:

(4)

English			*QF*				
⟨cla̲y̲⟩	e	(A · I̲)	⟨cri̲e̲r̲⟩	krie̲	e	(A · I̲)	'to scream'
⟨fo̲e̲⟩	o	(A · U̲)	⟨oise̲a̲u̲⟩	wazo̲	o	(A · U̲)	'bird'
⟨sa̲w̲⟩	ɔ	(U · A̲)	*	*	*ɔ	*(U · A̲)	
*	*ɛ	*(I · A̲)	⟨la̲i̲t⟩	lɛ̲	ɛ	(I · A̲)	'milk'

In order to explain the English data, Kaye [258] proposes the GC 'Nothing can license I'[5] and, for domain-final stressed vowels, that they must be dominated by a branching nucleus because of which they must be headed. Note that the GC 'I must be head' cannot be assumed for English because it makes the wrong prediction that (I), as in English ⟨si̲t, si̲n, bi̲t, bi̲n⟩, *etc.*, is ungrammatical.

For QF, Charette [85] proposes firstly that domain-final (unlicensed) nuclei have to be headed, and secondly that the occurrence of U is restricted by the GC 'U must be head'. The problem with finding a GC for the U-element in QF becomes evident when we also look at domain-internal vowels since we cannot propose 'PEs must be headed' for them. Word-medially, QF exhibits PEs at LL, *i.e.* 'underlying' PEs, where U appears as operator, *e.g.* (A · U) in ⟨vo̲ter⟩ vɔ̲te 'to vote' and (A · U · I) in ⟨beurrer⟩ bœ̲re 'to butter'. It is worth mentioning in this context that U cannot be operator at LL in a simplex PE, so (U)—which is not excluded by 'U must not be licensed' without 'PEs must be headed' or 'Operators must be licensed'—does not exist in the QF lexicon. Therefore neither 'U must be head' (as proposed by Charette)

[5] This GC was first published for English in Kaye [262].

nor 'Nothing can license U' (Kaye)—which is identical to 'U must not be licensed'—are possible options for non-domain-final vowels. I will leave the precise nature of the correct GC on the U-element for QF to future research. However, independently of that, I agree with Charette's proposal that in QF domain-final (unlicensed) nuclei must dominate headed expressions (*cf.* (1)) and must be U-headed. Consequently NVs, which *do* occur domain-finally in QF, must be headed too.

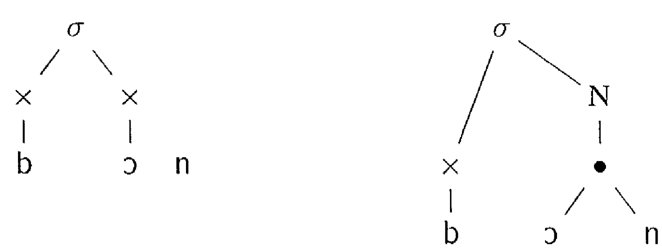
6.1.3.2 Proposal 2: QF NVs are attached to two nuclear positions

In this section, I will provide four arguments in favour of my claim that QF NVs are attached to two positions. In combination, they will also show that QF NVs are dominated by branching nuclei, not branching rhymes. These arguments involve, firstly, head-alignment as proposed by Charette [85] (section 6.1.3.2.1), secondly, distributional properties of NVs (6.1.3.2.2), thirdly, a restriction on the internal representation of PEs attached to a branching nucleus (6.1.3.2.3) and, finally, the p-licensing of empty nuclei right-adjacent (at P_N)[6] to NVs (section 6.1.3.2.4).

Note that this claim contradicts Prunet's proposal that domain-final NVs are derived from a structure as in (5) with a floating nasal /n/ which, due to a convention specific to French, "associates to the preceding Nucleus if it is not followed by an empty Onset" (Prunet [401, p. 227]):

(5) bɔ̃, *Prunet-style*

a. *Underlying form:* /bon/ b. *Surface:* [bɔ̃]



Opposed to Prunet's account, QF NVs are not derived in my analysis nor are they linked to one skeletal point but are, underlyingly and at the surface, attached to two points. However, in line with Prunet's view, I will claim below that for QF NVs, the element motivating nasality, *i.e.* L, is not fused with the aperture elements contained within the NV in question.

Finally, let me add here that in QF, as in many other varieties of French, there are no coda-onset sequences in which the coda dominates a nasal stop. This apparent

[6] 'P_N' stands for 'nuclear projection'.

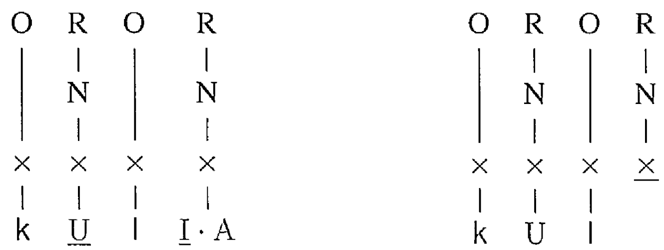
gap might suggest that QF NVs are derived from underlying nasal-obstruent clusters. This view would also be supported by the fact that, within one and the same phonological domain, there may be no NVs in QF which precede r j w or nasal consonants. An analysis of NVs as coda-onset sequences would account for this gap by saying that such transconstituent government domains, *e.g.* -mw- -nr-, are also ungrammatical in languages without NVs.[7] However, there is strong evidence which suggests that in spite of these gaps, QF NVs are attached to branching nuclei at LL.

Let me now present the arguments in favour of this claim.

6.1.3.2.1 Head-alignment Charette [85] proposes a phonological process for QF that changes a lexically headed PE attached to a single skeletal point into a headless one when it is adjacent to an empty nucleus to its right at P_N; this empty nucleus may or may not be a government-licensor,[8] *i.e.* there may only be a single (consonantal) skeletal point between (left-hand) target PE—changing from headed to headless—and (right-hand) trigger—*i.e.* a (p-licensed) empty nucleus:

(6)

a. ⟨couler⟩ kule 'flow (inf)' *(at LL/PL)* b. ⟨coule⟩ kul 'flows (3sg.pres)' *(at PL)*



(6a.) shows a headed PE at LL: ($\underline{U}$) in k$\underline{u}$le. The lexical representation of this PE is identical in the form of the 3sg present tense: (lexical) ($\underline{U}$) in (derived) k$\underline{u}$l. However, QF phonology changes this headed PE in (6b.) into a headless one when it immediately precedes an empty nucleus at the nuclear projection. Thus ($\underline{U}$) becomes (U) at PL.

Opposed to that, some QF vowels which sometimes sound slightly longer than

[7]Due to the existence of geminate nasal stops in Italian which, in line with proposals about Italian by Kaye [256] and KLV [267], appear to be linked to coda-onset sequences, this analysis would have to add the unproblematic proposal that geminate consonants are ungrammatical in some languages (including French). Such a claim would be unproblematic because the ungrammaticality of 'long' consonants is observable in other languages which do exhibit branching rhymes (*e.g.* in English).

[8]Regarding government-licensing, *cf.* Charette [82, 83].

other vowels are not affected by head-alignment ('HA'). (7a.) shows infinitive forms of verbs (*cf.* (6a.)), a context in which no HA can take place. In (7b.), on the other hand, all of the examples are domains whose rightmost unlicensed nuclei are in the correct context for HA (*cf.* (6b.)) even though no such process can be observed:

(7)

Infinitive			*3sg Present*		
⟨mâcher⟩	mɑːʃe	'chew'	⟨mâche⟩	mɑːʃ	'chews'
⟨sauter⟩	soːte	'jump'	⟨saute⟩	soːt	'jumps'
⟨bêler⟩	bɛːle	'bleat'	⟨bêle⟩	bɛːl	'bleats'
⟨jeûner⟩	ʒøːne	'fast'	⟨jeûne⟩	ʒøːn	'fasts'

Charette [85] claims that those vowels which do not undergo HA are 'long' at LL. In other words, PEs that do not get head-aligned are attached to a branching nucleus. We can explain this situation by saying that it is a universal constraint within GP (*cf.* Kaye [258]) that constituent governors must be headed. Thus a governor of a branching nucleus cannot be made empty-headed by HA.

This situation provides us with a test that indicates whether a PE linked to a nucleus is attached to one or two positions: If a PE can be affected by HA it must be lexically 'short'. If it cannot be affected, it must be lexically 'long'.

Looking at QF NVs (*e.g.* ʃã̲te), one observes that, opposed to short vowels (kru̲te), they *cannot* be affected by HA—just like non-nasal long vowels (so̲ːte):

(8)

⟨croûtée⟩	krute	'crusted'	⟨croûte⟩	krʊt	'crust'	*HA*
⟨sauter⟩	soːte	'jump (inf)'	⟨saute⟩	soːt	'jumps (3sg.pres)'	*no HA*
⟨chanter⟩	ʃãte	'sing (inf)'	⟨chante⟩	ʃãt	'sings (3sg.pres)'	*no HA*

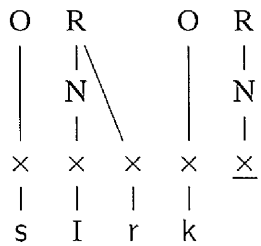
Consequently, I propose that NVs are attached to two skeletal points.

Furthermore, note that a domain-final nucleus which is a government-licensor, *i.e.* which follows a coda-onset sequence or a branching onset, can also trigger HA. So both the ɩ in sɩ̲rk (9a.) as well as the ʊ in sʊ̲pl (9b.) are in the right context for HA:[9]

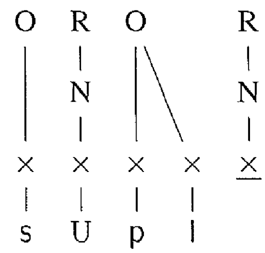
[9]Many QF speakers have no domain-final branching onsets; ⟨souple⟩ is sʊp for them. However, since HA is not sensitive to the licensing powers of the potential head-aligner, this is not relevant here.

(9)

a. ⟨cirque⟩ sɪrk 'circus' *(at PL)*



b. ⟨souple⟩ sʊpl 'supple' *(at PL)*



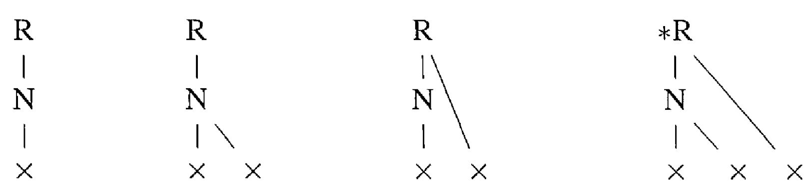
Since nuclei governing a coda *can* be head-aligned while only branching nuclei and those dominating NVs cannot, HA does not only provide evidence for the claim that QF NVs are attached to two rhymal points but also supports the view that they are linked to two nuclear points.

6.1.3.2.2 Distributional facts The second argument in favour of my proposal that QF NVs are dominated by two skeletal points involves the distribution of NVs. GP makes the claim that, universally, there are three possible rhymes: a non-branching rhyme dominating a non-branching nucleus (10a.), a non-branching rhyme dominating a branching nucleus (10b.) and a branching rhyme dominating a non-branching nucleus (10c.). Note that the postnuclear rhymal position, the so-called 'coda', can only occur if it is licensed by a following onset point ('coda'-licensing, *cf.* Kaye [254, p. 311]). A branching rhyme dominating a branching nucleus (10d.) is universally ill-formed (Binarity Theorem, *ibid.*, p. 306):[10]

(10)

Well-formed rhymes *Ill-formed rhyme*

a. b. c. d.

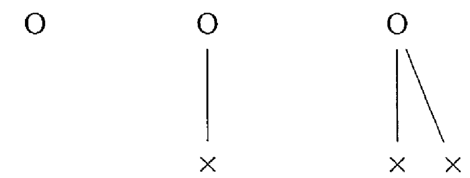


In addition, there are three types of onsets in GP: an onset dominating no skeletal point (11a.), a non-branching onset dominating one skeletal point (11b.) and a branching onset dominating two skeletal points (11c.):

[10]GPists subscribing to the 'strict CV' approach do not postulate rhymes or branching constituent. For them, any two nuclear points must belong to separate nuclei, *cf.* Lowenstamm [306] and van der Hulst & Rowicka [229].

(11)

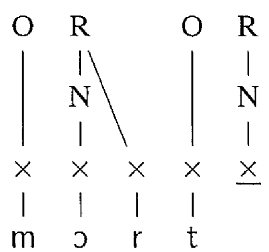
a. b. c.



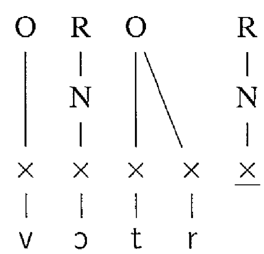
In QF, rhymes and onsets are licensed to branch, or, in other words (10c.) and (11c.) are well-formed in QF. Furthermore, domain-final empty nuclei are p-licensed in QF and are direct and indirect government-licensors. Thus the forms in (12) displaying short vowels preceding a 'coda'-onset sequence (12a.) or a branching onset (12b.) are grammatical:[11]

(12)

a. ⟨morte⟩ mɔrt 'dead (f)'



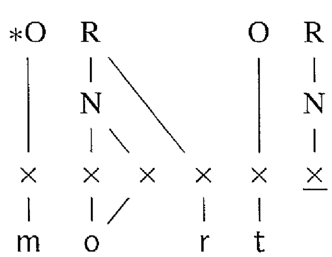
b. ⟨votre⟩ vɔtr 'your (pl, polite sg)'



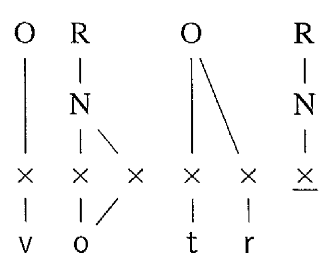
Since a branching nucleus within a branching rhyme is universally ill-formed, it is impossible to substitute the PE linked to one skeletal point in (12a.), *i.e.* ɔ (A · U), with the corresponding headed PE, attached to two points (13a.), *i.e.* o (A · <u>U</u>). On the other hand, to substitute a short ɔ in (12) with headed long oː is well-formed (13b.):

(13)

a. *moːrt



b. ⟨vôtre⟩ voːtr 'yours (pl, polite sg)'



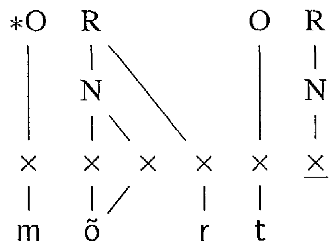
This distribution of PEs attached to two skeletal points provides us with another test for 'long' vowels, *i.e.* vowels attached to two rhymal points. PEs attached to one nuclear point can precede a coda-onset sequence or a branching onset; PEs attached

[11] As elsewhere I underline skeletal points (<u>×</u>) which are p-licensed at PL.

to two nuclear points *cannot* be followed by a coda-onset sequence. In QF, NVs cannot be followed by a postnuclear rhymal position:

(14)

*mõrt

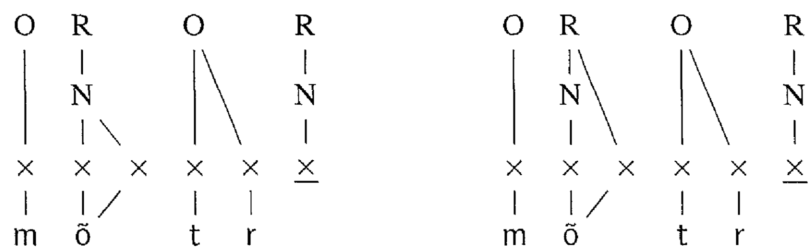


This leaves two possible structures for QF NVs, a branching nucleus within a non-branching rhyme (15a.) or a non-branching nucleus governing a postnuclear rhymal position within a branching rhyme (15b)[12]:

(15)

⟨montre⟩ mõtr 'shows (3sg.pres)' *(at LL)*

a. *or* b.



As I have shown above, HA suggests that QF NVs are linked to branching nuclei, not branching rhymes. It can therefore be said that the structure in (15b.) is incorrect. In sections 6.1.3.2.3 and 6.1.3.2.4 I will provide further evidence for this claim.

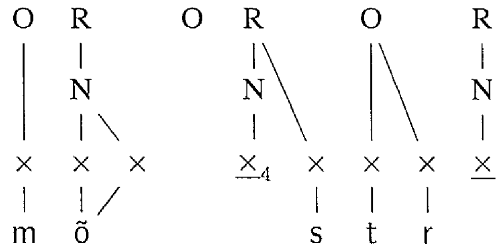
Note that there is one 'exception' to my claim that NVs cannot precede a postnuclear position: NVs followed by sC-clusters, where s is linked to a coda while any other following consonant governing s is dominated by the following onset point. For example, QF ⟨monstre⟩ mõstr 'monster'. Kaye [256], however, provides ample evidence for the special status of sC-clusters, so I propose the structure in (16) for mõstr:[13]

[12]Since I will ultimately reject an analysis of QF NVs as branching rhymes, I neglect here that there is a third possibility which is (almost) identical to the structure in (15b.) but in which the nasal element is not fused with the other elements present in the NV in question but is linked to the coda on its own while A, I and/or U are dominated by the governing nuclear point.

[13]$\times_4$ is p-licensed, *i.e.* licensed to be silent, via magic licensing (*cf.* Kaye [256]). P-licensed, *i.e.* silent, skeletal points are underlined ('$\underline{\times}$').

(16)

⟨monstre⟩ mõstr ‘monster’



To sum up, based on the observation that NVs are prosodically distributed like long oral vowels (‘OVs’) (or short OVs followed by a coda), I conclude that QF NVs are dominated by two rhymal points.

6.1.3.2.3 The must-contain-A constraint In this section, I will look at a constraint on the internal representation of ‘long’ OVs and show that NVs are subject to the same restriction.

In QF, non-nasal PEs that are linked to a branching nucleus at LL have to be headed (due to the universal constraint that governors of branching constituents have to be headed) and have to contain A.[14] As I have mentioned above, all headed PEs containing U as operator are excluded at LL by the GC ‘U must not be licensed’.[15] Assuming these GCs I expect QF to have five PEs that can be linked to a branching nucleus: ($\underline{\text{A}}$), (A · $\underline{\text{U}}$), (I · $\underline{\text{A}}$), (A · $\underline{\text{I}}$) and (A · I · $\underline{\text{U}}$). In order to explain the non-occurrence of long eː (A · $\underline{\text{I}}$) in French, Charette [85] proposes the GC ‘I as licensor of operators must not be linked to two positions’. So the four long QF OVs are:

(17)

($\underline{\text{A}}$)	⟨pâte⟩	pɑːt	‘pasta’
(A · $\underline{\text{U}}$)	⟨saute⟩	soːt	‘jumps (3sg.pres)’
(I · $\underline{\text{A}}$)	⟨bête⟩	bɛːt	‘beast’
(I · A · $\underline{\text{U}}$)	⟨émeute⟩	emøːt	‘riot’

The must-contain-A constraint on PEs attached to two nuclear points is not unique

[14]Note that this must-contain-A constraint does not apply to vowels whose length is not phonological, *i.e.* all stressed vowels preceding r z ʒ v: ⟨cerise⟩ səriːz ‘cherry’, ⟨peinture⟩ pɛ̃tyːr ‘paint’, ⟨rouge⟩ ruːʒ ‘red’, ⟨livre⟩ liːvr ‘book’.

[15]There can neither be a constraint on the A- or I-element because there could otherwise be only one headed PE containing A and I ((A · $\underline{\text{I}}$) *or* (I · $\underline{\text{A}}$)), a state of affairs proven wrong by the attested occurrence of two domain-final (and thus headed) PEs containing A and I (*cf.* 1). In addition, there is only one headed o-, one y- and one ø-type vowel; so in relation to domain-final and thus headed PEs in QF, this means that to predict two e-, one o-, one y- and one ø-type vowel, we need a GC on the U-element.

to QF. Another example can be found in Yawelmani, where an A-operator is added to any PE not containing A at LL (*cf.* Kuroda [280] for data, Ploch [383] for an analysis). Similarly, in Standard German not all PEs linked to a branching nucleus but those attached to governors of heavy diphthongs, *i.e.* a subgroup of Standard German branching nuclei, must contain A; for example, there is no uj in Standard German while there is aj aw oj.

As pointed out above, not only oral vowels attached to two nuclear points must contain A, QF NVs do too. Since the must-contain-A constraint is operative for OVs at LL, it is a reasonable assumption that QF NVs are attached to two nuclear points at LL. Note also that the must-contain-A constraint does not affect oral nuclear governors of codas, *e.g.* in ⟨cirque⟩ sɪ̱rk 'circus', or derived long vowels, *i.e.* stressed vowels preceding r z ʒ v (⟨peinture⟩ pɛ̃ty̱ːṟ). It follows that QF NVs are not linked to a nucleus and its postnuclear rhymal position but are attached at LL to two nuclear points, *i.e.* a branching nucleus.

6.1.3.2.4 P-licensing I have shown above that QF NVs match vowels linked to rhymes dominating two positions in relation to their phonological behaviour. In addition, QF HA (section 6.1.3.2.1) and the must-contain-A constraint (6.1.3.2.3) provide evidence that NVs are part of the QF lexicon, *i.e.* they are not derived, and that they are attached to branching nuclei, not to branching rhymes. In this section, I will discuss another phenomenon which points to an analyis of QF NVs as branching nuclei: p-licensing.

More specifically, the *non-realisation* of empty nuclei immediately following heads of branching nuclei at P_N or of those following NVs, in opposition to the *realisation* of empty nuclei following branching rhymes suggests that NVs are dominated by branching nuclei, not branching rhymes. Consider the following data:[16]

[16]I will neglect here that ⟨amusement, soulagement⟩ in (18d.) contain derived long vowels—which are therefore not subject to the must-contain-A constraint operative at LL—and how such vowels are to be represented in GP, particularly without violating the Projection Principle ("Governing relations are defined at the level of lexical representation and remain constant throughout a phonological derivation", *cf.* KLV [267, p. 221]). Note that I will also not discuss why, contrary to GP's predictions, the leftmost empty nucleus in ⟨acẖemine-ment⟩ is not p-licensed, *i.e.* not silent. I suspect that French ʃ is phonologically motivated by one PE dominated by the two points of a coda-onset sequence.

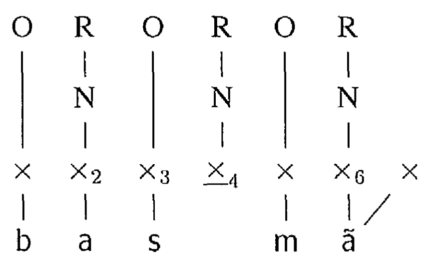
(18) *P-licensing and ⟨-ment⟩ in Québec French (data)*

a.	⟨achemine-ment⟩	aʃəmɪn-mã	'conveyance'
	⟨actionne-ment⟩	aksjɔn-mã	'drive'
	⟨ruisselle-ment⟩	rɥisɛl-mã	'trickling'
	⟨soubasse-ment⟩	subas-mã	'base'
b.	⟨ameuble-ment⟩	amœblə-mã	'furnishings'
	⟨sacre-ment⟩	sakrə-mã	'sacrament'
	⟨accouple-ment⟩	akuplə-mã	'clutch, coupling'
	⟨accoutre-ment⟩	akutrə-mã	'outfit'
c.	⟨ajourne-ment⟩	aʒurnə-mã	'summons'
	⟨apparte-ment⟩	apartə-mã	'appartment'
	⟨bombarde-ment⟩	bõbardə-mã	'bombardment'
	⟨gouverne-ment⟩	guvɛrnə-mã	'government'
d.	⟨bêle-ment⟩	bɛːl-mã	'bleating'
	⟨affaisse-ment⟩	afɛːs-mã	'sagging'
	⟨amuse-ment⟩	amyːz-mã	'amusement'
	⟨soulage-ment⟩	sulɑːʒ-mã	'relief'

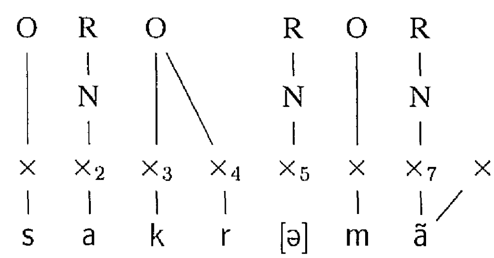
To explain the data in (18), let me first provide the relevant constituent structures of the forms in (18a.–d.):[17]

(19) *P-licensing and ⟨-ment⟩ in Québec French (structures)*

a. (su)bas-mã

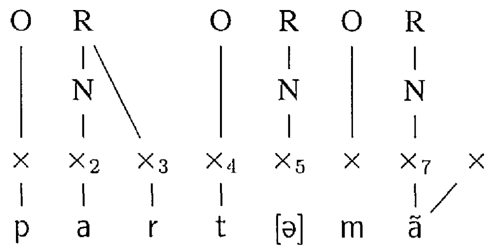


b. sakrə-mã

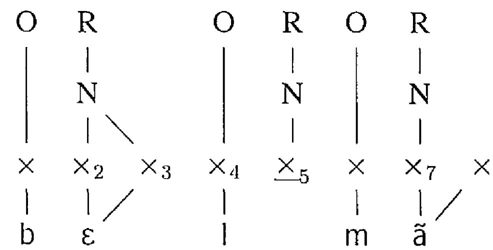


[17]The final skeletal point (dominating ã) is indicated as unlinked to either rhyme or nucleus while it is attached to a PE, here the one represented by ã. This is not possible in any version of GP that I am aware of. The relevant link is not given since it is the very issue under discussion here. Also note that in French, unlicensed, *i.e.* audible, empty nuclei are realised [ə] when unstressed, [ɛ] when stressed; however, such nuclei are empty at LL (*cf.* Charette [83]).

c. (a)partə-mã



d. bɛːl-mã



The vowel-zero alternations preceding -mã depend on the licensing properties of the nucleus exhibiting these alternations. This explanation was developed (without specific reference to the -mã-suffix) by KLV [267] and Charette [82, 83]. I will not repeat the argumentation here. It suffices to say that in GP empty nuclei are subject to the Phonological Empty Category Principle ('ECP'). This principle states that "a p-licensed (empty) category receives no phonetic interpretation" (Kaye [256]).[18]

In (19a.), $\times_6$ properly governs $\times_4$ which is therefore p-licensed and thus silent. In (19b.), $\times_7$ is a proper governor of $\times_5$. However, since in QF domain-internal p-licensed empty nuclei are no government-licensors (neither direct nor indirect ones), $\times_5$ does not get p-licensed—and is therefore audible—in order for it to be able to license $\times_3$ to (intraconstituently) govern $\times_4$. Similarly, in (19c.), $\times_5$ gives $\times_4$ the license to (transconstituently) govern $\times_3$; because of this, $\times_5$ is phonetically realised, *i.e. not* p-licensed. As in (19a.–c), the nucleus preceding -mã in (19d.) is properly governed; but only as in (19a.) but not in (19b.–c.), it is not a government-licensor and silent.

This provides us with a test for the differentiation of branching nuclei from branching rhymes: empty nuclei immediately preceding -mã at the skeleton—which are properly governed by the nucleus attached to the rhyme dominating ã—may only be p-licensed and thus be silent if they are no government licensors. This means that such silent empty nuclei may not be immediately preceded by a branching onset or a coda-onset sequence. Relevant for the construction of our test is that the rhyme preceding a p-licensed empty nucleus does not branch in QF. Let us now look at examples where the rhyme preceding a properly governed empty nucleus dominates a NV ((20) for data, (21) for the relevant representation):

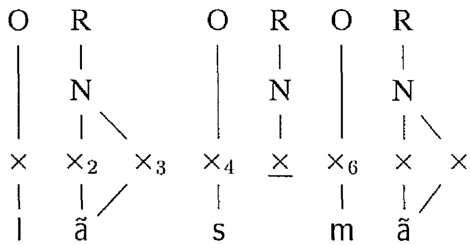
[18]An early version of the ECP only relating to empty nuclei was formulated in Kaye [255]. Drawing on Kaye [256, 258], *i.e.* according to the a version of the ECP revised to 1993, there are four types of p-licensed categories: 1. a domain-final empty nucleus p-licensed by parameter, 2. a properly governed category (Charette [82, 83]), 3. a nucleus within an inter-onset domain (Kaye [259]) and 4. a magically licensed nucleus (Kaye [256]). For further discussion, *cf.* Charette [84] and Gussmann & Kaye [190].

(20)

⟨prolonge-ment⟩	prɔlõʒ-mã	'extension, prolongation'
⟨amende-ment⟩	amãd-mã	'ammendment'
⟨arrange-ment⟩	arãʒ-mã	'arrangement'
⟨balance-ment⟩	balãs-mã	'balancing'

(21)

(ba)lãs-mã



Based on the above it can be said that, if in QF, NVs may be followed by silent empty nuclei (which are in the right phonological context to be properly governed), they cannot be attached to a branching rhyme. Since empty nuclei as in (20)—which are properly governed by the nucleus dominating ã—are silent when preceded by NVs, it follows that QF NVs are attached only to a nucleus, not to a coda. In combination with the evidence gained by a study of HA, distributional facts and the must-contain-A constraint this means that QF NVs must be linked to branching nuclei at LL.

As pointed out above, there are no coda-onset sequences in QF in which the coda dominates a nasal stop. This claim is supported by the fact that QF exibits other types of coda-onset sequences preceding (silent) domain-final empty nuclei, *e.g.* ⟨cirque⟩ sɪrk∅ 'circus', ⟨veste⟩ vɛst∅ 'jacket', but no words like *ciŋk∅ or *vent∅. It follows that in the few examples in which the acoustic cue of a 'nasal stop' precedes the acoustic cue of a PE which could coda-license[19] it in other languages (*e.g.* in English ⟨hand⟩ hænd∅ or (Southern) German ⟨Hand⟩ hant∅), these two PEs are not phonologically adjacent at the skeleton but are separated from each other by an empty nucleus: ⟨caneton⟩ kan∅tõ 'little duck'. Since it can be said that all QF nasal-obstruent 'clusters' display an intervening (licensed, *i.e.* silent) empty nucleus, the non-occurrence of coda-onset sequences with the coda linked to a nasal stop might suggest that QF NVs are derived from such coda-onset clusters. This, however, is

[19]Regarding ' "coda" licensing', *cf.* Kaye [254].

contrary to the evidence put forward here: I have shown that QF NVs cannot be linked to coda-onset sequences but must be attached at LL to branching nuclei. The apparent gap, *i.e.* the non-existence of nasal stops attached to postnuclear rhymal positions, must either be regarded as a historical accident or remain unexplained (for now).[20]

6.1.4 Independent evidence: a unified account of QF NVs, MF VN- and English NC-sequences

In this section, I will show that QF NVs, MF VN- and English NC-clusters can be analysed as sequences of an oral vowel followed by an onset dominating N (*i.e.* (ʔ · L)). This will not completely overturn the analysis argued for above; I will continue to claim that QF NVs contain a branching nucleus. What I will change is that I will propose in the following that in QF, L (motivating phonetic nasality of a QF NV) is not fused with the rest of the melodic make-up of a NV.

To start the discussion, let us look at Montpelier French from southern France. In MF, QF NVs correspond to VN-clusters, *e.g.* QF õb comes out as omb in MF. This will show that in addition to QF, there are other varieties of French in which that which is a NV in QF is attached to two rhymal points.

Consider the data in (22):

(22)

	QF	*MF*	*Gloss*
⟨pâte⟩	pɑːt	patə	'pasta'
⟨patte⟩	pat	patə	'paw'
⟨jeûne⟩	ʒøːn	ʒœnə	'fasts (3sg)'
⟨jeune⟩	ʒœn	ʒœnə	'young'
⟨saute⟩	soːt	sɔtə	'jumps (3sg)'
⟨sotte⟩	sɔt	sɔtə	'dumb (f)'
⟨bête⟩	bɛːt	bɛtə	'beast'
⟨cette⟩	sɛt	sɛtə	'that (f)'

As pointed out in section 6.1.3.2.1, in QF, PEs linked to a branching nucleus at LL cannot be head-aligned. (22) illustrates that for MF OVs, the distinctions between branching versus non-branching nuclei does not exist, since all of the vowels have

[20] I am currently preparing an article in which I will discuss this problem in more detail [377].

the same length. Consequently, all MF OVs are dominated either by a non-branching or a branching nucleus. However, to my knowledge, there is no language that has only branching nuclei but no non-branching ones. Therefore, let me propose that, neglecting the MF correspondences of NVs attested in other 'varieties of French', MF does not permit nuclei to branch.

Let me now add that MF exhibits no NVs; QF NVs correspond to MF VN-sequences:

(23)

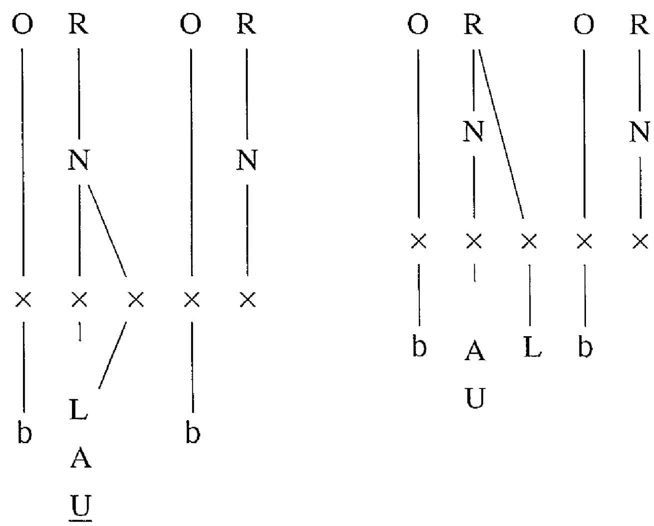
			QF	*MF*	*Gloss*
a.	ã ~ aN	⟨jambe⟩	ʒãb	ʒambə	'leg'
		⟨jambière⟩	ʒãbjɛːr	ʒambjɛrə	'leggings, gaiter'
		⟨menthe⟩	mãt	mantə	'mint'
		⟨cantine⟩	kãtsɪn	kantinə	'refectory'
		⟨banque⟩	bãk	baŋkə	'bank'
		⟨banquet⟩	bãkɛ	baŋke	'banquet'
b.	õ ~ ɔN	⟨bombe⟩	bõb	bɔmbə	'bomb'
		⟨bombarder⟩	bõbarde	bɔmbarde	'to bombard'
		⟨monde⟩	mõd	mɔndə	'world'
		⟨mondial(e)⟩	mõʤjal	mɔndjal	'word-wide'
		⟨longue⟩	lõg	lɔŋgə	'long (f)'
		⟨longueur⟩	lõgœːr	lɔŋgœrə	'length'
c.	ø̃ ~ œN	⟨humble⟩	ø̃b(l)	œmblə	'humble'
		⟨humblement⟩	ø̃bləmã	œmbləmaŋ	'with humility'
		⟨jungle⟩	ʒø̃g(l)	ʒœŋglə	'jungle'
d.	ẽ ~ ɛN	⟨simple⟩	sẽp(l)	sɛmplə	'simple'
		⟨impossible⟩	ẽpɔsɪb(l)	ɛmpɔsiblə	'impossible'
		⟨sainte⟩	sẽt	sɛntə	'holy (f)'
		⟨peinture⟩	pẽtsʏːr	pɛntyr	'paint'
		⟨seringue⟩	srẽg(l)	sərɛŋgə	'syringe'
		⟨cinquième⟩	sẽkjɛm	sɛŋkjɛmə	'fifth'

Based on the data in (23), we could try to claim that QF NVs correspond to branching rhymes in MF. In other words, both varieties would have in common that what is a NV in one variety is linked to two rhymal skeletal positions in both varieties.

The only difference would be that the elements which are dominated by a branching rhyme in MF (24b.) are fused in QF within one PE and attached to one nucleus (24c.):

(24) ⟨*bombe*⟩ *'bomb' in QF and MF (to be revised)*

a. *QF* bõb   b. *MF* bɔmbə



The apparent parallel between QF and MF in relation to the number of skeletal points a NV (QF) or nucleus-coda sequence (MF) is attached to does in no way imply that all varieties which are considered varieties of French have to exhibit this parallel. This does not mean that there is not something to be said about why NVs appear to be phonologically 'long' in many languages (*cf.* Hajek [196, pp. 116ff.] and above, chapter 3). I doubt, however, that phonetic measurements will have any contribution to make to the explanation of this correlation. A consequence of this view is that there might well be biolects—commonly regarded 'varieties of French'—which exhibit NVs that are only linked to one nuclear point. Further research will clarify this.[21] Importantly, the parallel between QF and MF cannot be employed as evidence within an analysis of QF or MF. My analysis of QF NVs as branching nuclei is independent of data taken from other 'varieties of French'.

However, even though it is common practice in versions of GP which propose a constituent rhyme and parameterised branching for all three constituent onset, nucleus and rhyme to assign NC-clusters in German, English, Polish, *etc.* a coda-onset sequence (as in (24b.))—because of which such an analysis might also be employed for MF—, there is a strong argument against doing so: If such an analysis is extended

[21] Jean Lowenstamm [p.c.] pointed out to me that, opposed to his variety, the French of younger Parisians does not appear to exhibit either 'long' nasal or 'long' oral vowels.

to MF, V in VN-sequences would exhibit the must-contain-A constraint even though this restriction would not be operative for V dominated by the rhymal head of any other postnuclear rhymal position. For example, while r and l in coda position can be constituently governed by a nucleus dominating i (⟨cirque⟩ sirkə 'circus') or y (⟨culte⟩ kyltə 'cult'), V preceding N in coda-position must contain an A-element in MF, *e.g.* in baŋkə, bɔmbə, œmblə, sɛmplə; so only a ɔ œ ɛ are possible in this position. Domain-final MF examples are vaŋ 'wind', sɔŋ '(they) are', œŋ 'a (m.)', vɛŋ 'wine'. I want to propose therefore that VN-sequences (preceding another C) and Vŋ-clusters in final position are dominated by a branching nucleus, just like QF NVs, which are also subject to the must-contain-A constraint.[22]

Let me point out though that my claim is not merely that QF NVs and MF VN-sequences are branching nuclei. It is true that we could declare the different realisations in QF and MF of one and the same prosodic structure to be phonetic detail and therefore irrelevant to the phonology. However, this would not explain why in MF, domain-final N (not any nasal consonant following an oral vowel!) is phonetically realised [ŋ] and not [n m], *etc.*, and why both QF NVs and MF heavy diphthongs can, within one and the same domain, not be immediately followed by a nasal consonant or w j r. Most importantly, there is no theoretical mechanism in GP on the basis of which we could formulate a constraint which would substantively restrict an onset dependent on the nucleus it follows; in other words, any onset O_2 should be well-formed after any nucleus N_1.[23]

It seems, what we need for both QF NVs and MF VN-sequences is a prosodic representation which allows us to assign the PEs involved to branching nuclei, on the one hand, and, at least in parts, to a following onset position which can be interonset-

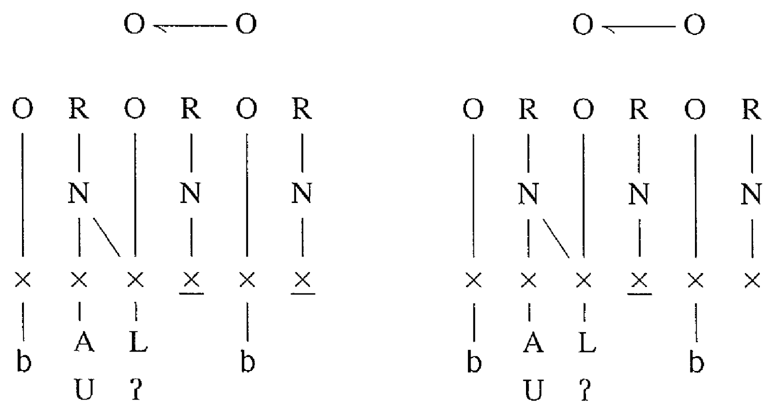
[22]I will neglect here whether QF and MF differ from each other with respect to the precise nature of the branching nuclei involved, for example, whether both languages exhibit fused NVs or whether one or both have nasal HDs with L associated only to the right-hand recessive position. I will discuss this matter in more detail below.

[23]The only restriction of this kind has been proposed by Harris [199, 203] in form of his notion 'licensing inheritance'. For example, onsets *following* stressed vowels exhibit well-formedness constraints that are not attested for onsets immediately *preceding* a stressed vowel. Since in such cases, the nucleus licensing a restricted onset (to its left) is itself licensed by the stressed nucleus, its licensing potential is weaker because of which the onset that a licensed nucleus licenses is more restricted. This neutralisation of contrast is particularly far-reaching and common when the licensed nucleus (providing the 'weak' license) is p-licensed. However, such an explanation would not work for the phonotactic constraint observable in QF (or MF) that a NV (or VN-sequence) may not be followed by a nasal consonant or w j r, since this restriction operates independently of whether the nucleus following (and licensing) N or w j r is p-licensed. For example, N w j r cannot follow a QF NV when the nucleus following N w j r is filled (*Ṽ rV) or when it is p-licensed (*Ṽr#). Similarly, in MF, a VN cannot be followed by N w j r; both *VNrVand *VNr# are ill-formed.

governed by a following onset (if present). The structures I want to propose for QF bõb and MF bɔmbə (*cf.* (24)) can be found in (25):

(25) ⟨*bombe*⟩ *'bomb' at LL in QF and MF*

a. *QF* bõb　　　　　　　b. *MF* bɔmbə



In both (25a.) and (25b.), there is a PE (not containing L) which is attached to the head of a branching nucleus (N_1). (ʔ · L) is linked to a skeletal point which is dominated simultaneously by two constituents, *i.e.* by N_1—thus forming the recessive (right-hand) position of a branching nucleus—and by the onset following N_1. To my knowledge, it has not been proposed before within GP to account for NC-clusters or for (certain) NVs in this manner.[24] Without simultanous attachment of N to a nucleus and the following onset, there would be no theoretical possibility to account for the simultaneous presence of the must-contain-A constraint—which is only supported by independent evidence for lexically branching nuclei in QF (or any other language, for that matter)—and the apparent licensing relationship from the onset following N towards N. As pointed out, even though branching rhymes do provide some kind of formalism which connects a nucleus with the onset it precedes via an intervening coda position, there is strong evidence against analysing QF NVs or MF VN-sequences as branching rhymes.

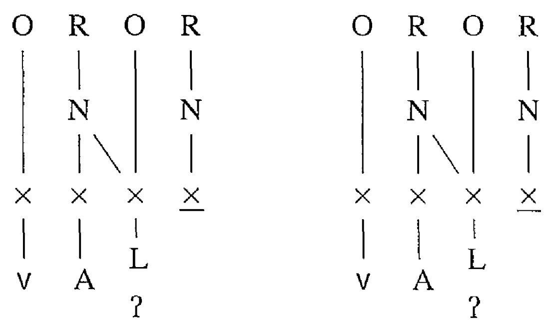
Note also that the well-formedness of the licensing relationship holding between the onset following N and N is not, as in Polish (*cf.* Gussmann & Kaye [190]), dependent on whether the nucleus licensing the right-hand onset (*i.e.* the inter-onset

[24]It is unclear whether a ʔ-element can only be linked to onsets and onset licensees. Even though there does not seem to be much evidence for allowing ʔ to be dominated by a nuclear point, for the purposes of this thesis, let me propose that in (25a.), the ʔ-element in N (ʔ · L) is only linked to O_2 (but not to N_1) while L is dominated both by L and ʔ. For those who would like to eliminate the ʔ from the set of proposed elements, let me add that there is no point in claiming that one's theory can do without the ʔ as long as one does not provide an alternative analysis of all the data because of which (present) supporters of the ʔ assume it in the first place.

licensee) is p-licensed. So the difference between the representations in (25) with respect to the p-licensing status of the domain-final empty nucleus has no influence in QF or MF on the inter-onset licensing relationship exhibited there. Furthermore, as the forms with a domain-final NV (QF) or VN-sequence (MF) in (26) show, an onset dominating N does not have to be inter-onset licensed; it merely has to be in such a relationship with any onset it immediately precedes at the onset projection. This explains why N in MF is velar, *i.e.* without an aperture element, when domain-final and not inter-onset licensed, but homorganic to the following onset when such is present. In the latter case, the licensing relationship between the following onset (licenser) and N (licensee) is made manifest by the constraint that the aperture element attached to the onset licenser at LL spreads at PL to the licensee.

(26) ⟨*vent*⟩ *'wind' in QF and MF*

a. *QF* vã b. *MF* vaŋ



As my analysis shows, the proposal of heavy diphthongs for QF and MF—as put forward by me in chapter 4—in combination with the assumption of simultaneous linkage of a point dominating N (*i.e.* (ʔ · L)) to the recessive branch of a branching nucleus and to the following onset is only motivated cognitively but not by the properties of a phonetically defined system.

Interesting for future research is finding an answer to the question why neither QF nor MF exhibit NC-sequences with N linked to a postnuclear rhymal position and C to the following onset. Remember that one of the proposals I dismissed was that QF NVs and MF VN-clusters are linked to rhymal-onset sequences. The reason was that such a analysis would not explain the apparent application of the must-contain-A constraint. Let me therefore suggest that also in English or other languages for which one might want to propose coda-onset sequences for NC-clusters that such languages disallow NC-clusters to be dominated by rhymal-onset sequences. Rather, let us say that, universally, NC-clusters (linked to two separate points) are in an inter-onset relationship.

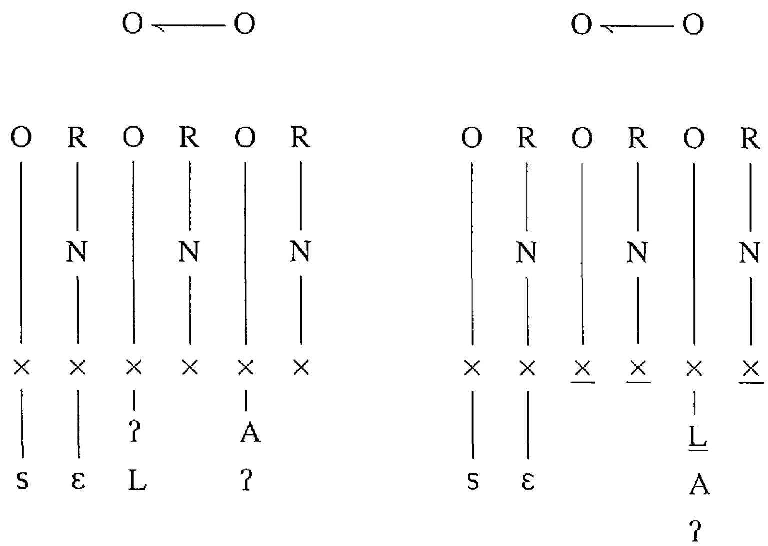
I have already shown in my explanation of Dahl's Law that voiced stops in C-positions behave differently from voiceless stops due to the fact that voiceless stops (or obstruents) contain H and the cross-linguistically common L/H-incompatibility. Interesting in this context is that, recently, Kaye [263] has proposed that NC-clusters with voiceless stops attract stress in English while those with voiced stops do not. While Kaye is of the opinion that voiced NC-clusters are one and the same L-headed PE while voiceless ones are rhymal-onset sequences, my research suggests that Kaye is right in his assumption about voiced NC-sequences but that voiceless ones are linked to two onsets separated by an empty nucleus which is p-licensed by the inter-onset relationship holding between these two onsets. As in the case of Meinhof's Law, the melodic material of N tries to move to the following onset. So in English, a voiced NC-sequence consists in its underlying representation of two onsets (O_1 and O_2); O_1 dominates N (ʔ·L), O_2, to the right, dominates a neutral stop, say, d (ʔ·A). At PL, the melodic material of N moves to O_2 and fuses with it, resulting in ^{n}d $(ʔ \cdot A \cdot \underline{L})$ attached to O_2 and O_1 empty. The resulting empty syllable (containing O_1 and its nuclear licenser N_1) is well-formed since its onset point is inter-onset licensed by the O_2 (*cf.* the ESC in (26), chapter 5, p. 224; *cf.* (27)).

Let me make it very clear here that I consider head-final inter-onset relationship of N (if followed by another onset) a universal property of N, which requires a specific stipulation in relation to ON-pairs containing an empty nucleus licensing an onset dominating N. Since I am providing evicence for such a claim, this is not problematic. If I, however, claimed at the same time that the phonology is phonetically motivated—which, dependent on the particular version of the PH invoked—may or may not include a phonetic 'explanation' for the special behaviour of ON-pairs containing N, this claim would be unfalsifiable: If I claimed that the phonology is motivated phonetically and that I have found a phonetic grounding for the phonological behaviour of N, it would have to remain unclear why there are numerous languages which ignore this phonetically motivated force; my version of the PH would exhibit a low degree of falsifiability. If I claimed, on the other hand, that the phonology is phonetically motivated but that the special properties of N are not, I would have to allow myself not to apply the PH whenever it does not work—as in the first case—which would entail a low degree of falsifiability too.

The following constituent structures will illustrate inter-onset licensing of N by a voiced stop:

(27) *English* ⟨*send*⟩ sɛnd

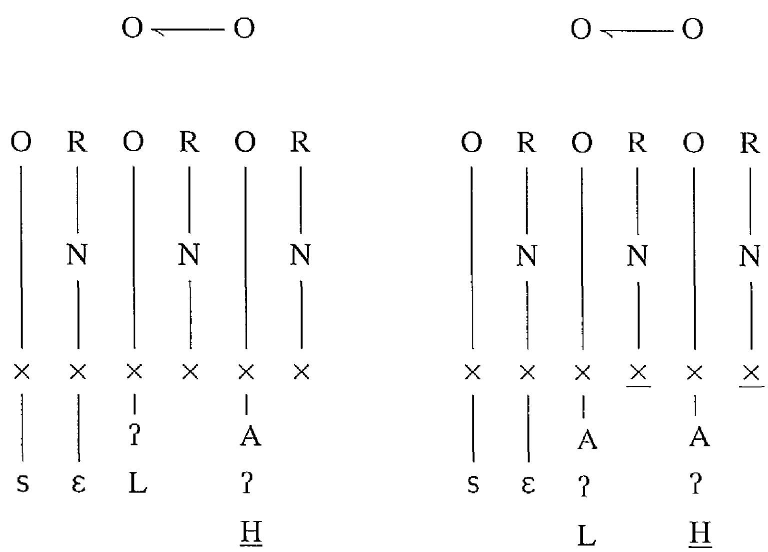
a. *at LL* b. *at PL*



In the case of an English NC-cluster with a voiceless C, say nt, on the other hand, N in O_1 cannot fuse with the PE dominated by O_2 because C in O_2 contains an H which is incompatible with L (*cf.* (28)). In addition, as in Bemba, the aperture element linked to O_2 (here A) spreads leftwards to the preceding onset. This means that, at PL, either N moves to the following onset or the onset following N spreads its aperture element to the preceding N:

(28) *English* ⟨*sent*⟩ sɛnt

a. *at LL* b. *at PL*



To sum up, I have proposed a unified account for QF NVs, MF NC-sequences and domain-final Vŋ-clusters in MF, and for those sequences which in versions of GP with branching rhymes are analysed as homorganic NC-clusters attached to a rhymal-onset sequence. All of these prosodic structures exhibit a nucleus followed by onset dominating (ʔ · L) which in turn, if followed by another onset, is inter-onset licensed by that onset; the intervening empty nucleus (licensing the onset dominating (ʔ · L)) is p-licensed by this inter-onset relationship. In QF and MF, N is additionally linked to the preceding nucleus which explains why the must-contain-A constraint operative in QF and MF is applied to QF NVs and MF VN-sequences. NC-sequences in coda-onset position have been analysed as onset-onset structures between which there is the same inter-onset licensing relationship holding that I also proposed in my account of the Bemba N-prefix (*cf.* chapter 5). Notice again that no discussion of the phonetics involved helped in any way to come up with this set of assumptions.

Having provided an analysis of the prosodic structure involved in NVs which are subject to the must-contain-A constraint, I will, in the following section, discuss the internal representation of QF NVs.

6.1.5 The internal representation of QF NVs

In this section, I would like to address whether in QF 'nasality' in PEs dominated by a nucleus is (part of) the phonetic realisation of L as operator or head of that PE.

To start, let me restate a number of constraints operative in QF: Firstly, NVs are dominated by branching nuclei at LL because of which they—like all long oral vowels in QF—have to contain A and must be headed. For domain-final (headed) nuclear PEs, there is a GC 'U must not be licensed'. Thirdly, the restriction 'I as licensor of operators must not be linked to two positions' operative at LL and PL precludes *eː, *i.e.* (A · $\underline{\mathrm{I}}$) linked to a branching nucleus, on both levels of representation.

In addition to that, let me now make two mutually exclusive assumptions and test their predictions:

(29) *Two assumption regarding the internal representation of QF NVs*

Assumption 1 Nasality in QF NVs is the realisation of L as *head* of a PE dominated by a nucleus.

Assumption 2 Nasality in QF NVs is the realisation of L as *operator* of a PE dominated by a nucleus.

If we now take into account the constraints operative in QF restated above, each of these assumptions makes different predictions about what and how many NVs must occur in QF (*cf.* (30)).

As long as we hypothesise that in QF NVs, L is fused with the other elements that are part of the melodic make-up of an NV, assumption 1 predicts two NVs: ã and ẽ. Note that all NVs not containing A are excluded by the must-contain-A constraint, while NVs containing U are ungrammatical because of the GC 'U must not be licensed'. Assumption 2, on the other hand, predicts precisely the following four NVs: ã ẽ õ ø̃. All of these NVs contain A; those which contain U contain U as head, and $\tilde{e}_2$ (A · L · <u>I</u>) is excluded by the restriction that 'I as licensor of operators must not be linked to two positions':

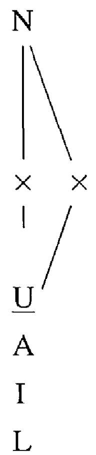
(30)

	NVs predicted by	
Symbol	*Assumption 1*	*Assumption 2*
	"L as head"	"L as operator"
ã	(A · <u>L</u>)	(L · <u>A</u>)
ẽ	(I · A · <u>L</u>)	(I · L · <u>A</u>)
õ	*	(A · <u>U</u>)
ø̃	*	(A · I · L · <u>U</u>)

What I have shown is that if I assume the constraint for oral branching nuclei to hold true for NVs, assumption 1 makes wrong predictions about QF, while assumption 2 predicts precisely those four NVs which actually occur in QF. Therefore I proposed in [380] that nasality in QF NVs is the realisation of L as operator, not as head, of a PE dominated by a nucleus.[25] The resulting lexical (and phonological) representation of a QF NV would look like the following: (example: ø̃):[26]

[25]In Ploch [380, p. 100], I also (mistakenly) proposed that, *universally*, NVs contain L as operator.
[26]The nucleus in (31) is dominated by a rhyme which has been omitted.

(31) *The internal representation of QF NVs (to be revised). Example: ø̃ at LL/PL*

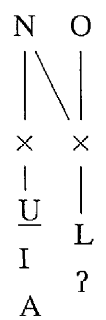


The problem with such an explanation would be that, if NVs can be motivated by a nuclear L-operator, L-heads would have to be assumed to be the cognitive motivation of low tone/pitch—alongside H-heads motivating high tone/pitch. However, ATR-harmony is seen in Element Theory to be the phonetic realisation of h(ead)-licensing (*cf.* Kaye [258], Walker [485], Cobb [110]); tense vowels are headed, lax vowels are headless. ATR-harmony is the phonological H/O role change from (lexical) headless to headed. Since in languages with both tones and ATR-harmony, tones have no influence on ATR-harmony, tones cannot be assumed to be cognitively motivated by L/H-*heads* but *-operators*. So if we want to continue to support the merger of L and N—argued for in chapter 5—nasality in vowels must be motivated by an L-head (assumption 1). This in turn does cannot predict the NVs attested in QF.

The solution to this problem lies in the fact that, in my analysis of QF NVs that I would like to present here, L is *not* fused with the melodic material dominated by the nuclear head of an NV. Therefore, L can be operator of the PE dominated by the recessive point of a branching nucleus. Opposed to fused NVs, where an L-operator is interpreted as low tone/pitch, in heavy NDs as in QF (or MF), an L-operator is phonetically realised as nasality, just as in the case of onsets. What onsets and recessive positions of branching nuclei have in common is, of course, that both are licensed by a nuclear head. Note also that the must-contain-A constraint must apply even though L is not fused with the rest of the melodic material. The reason for that is that the (parameterised) must-contain-A constraint is not sensitive to the precise nature of the elements linked to the same nucleus. Its application is triggered by any branching nucleus.

It follows that my analysis of QF NVS cannot only explain the fact that onsets following NVs are restricted and that, simultaneously, the must-contain-A constraint applies, but also the question why the must-contain-A constraint is taken into account by the GCs generating the lexical set of nuclear PEs while the presence of L is not. This way, we only predict nasal versions of all oral vowels which can be linked to a branching nucleus at LL in QF, *i.e.* of a ɛ o œ, resulting in four lexical NVs each consisting of two PEs linked to separate points as indicated. Neglecting the ʔ-element, these are: ã ($\underline{A}$)-(L), ɛ̃ (I · $\underline{A}$)-(L), õ (A · $\underline{U}$)-(L), and ø̃ (A · I · $\underline{U}$)-(L). As example, I provide the prosodic structure of ø (*cf.* (31)):[27]

(32) *The internal representation of QF NVs. Example: ø̃ at LL/PL*



To sum up, I have proposed a unified cognitive account of QF NVs, MF VN- and English (and other) NC-sequences which are usually analysed as rhymal-onset sequences or a 'strict CV'-type notational variant thereof. This analysis also contained an explanation of the French version of the Heightmyth.

6.2 The neutralisation of tense-lax contrasts for nasal vowels in Brazilian Portuguese

Having discussed the phonological reasons why QF NVs and the vowels in MF VN-sequences must be non-high, let me now put forward an explanation of the apparent neutralisation of tense-lax distinctions in Portuguese NVs which are observable for OVs. This account will show that the phonetics involved cannot motivate the phonology. The data I will employ to illustrate this is from Brazilian Portuguese (from São Paulo, henceforth 'BP').[28]

BP exhibits the following seven stressed lexical OVs:

[27]As elsewhere where possible, the rhyme dominating the nucleus in (32) has been omitted. Also, in line with the restriction that ʔ cannot be linked to nuclei (probably universally), in (32), ʔ is only linked to the onset, not the preceding nucleus, and only L is attached to both nucleus and onset.

[28]The data was kindly given to me by Angelica Sampaio, a native speaker working on Portuguese; *cf.* Sampaio [428].

(33) *Stressed OVs in Brazilian Portuguese*

i	⟨livro⟩	l<u>i</u>vru	'book'
u	⟨bula⟩	b<u>u</u>lɐ	'compass'
a	⟨bala⟩	b<u>a</u>lɐ	'bullet'
e	⟨dedo⟩	d<u>e</u>du	'finger'
ɛ	⟨belo⟩	b<u>ɛ</u>lu	'beautiful (m.)'
o	⟨bolo⟩	b<u>o</u>lu	'cake'
ɔ	⟨bola⟩	b<u>ɔ</u>lɐ	'ball'

Ignoring NVs for the moment, we can say that the smallest set of GCs generating this system is the one given in (34):[29]

(34) *GCs for stressed nuclei in Brazilian Portuguese (ignoring NVs)*

Operators must be licensed.

Remember that the fusion and the I/U-fusion parameter are in my account switched OFF by default. In BP, acquirers only have evidence to switch the fusion parameter ON, but not the I/U-fusion parameter. In combination with 'Operators must be licensed', we get the following system:

(35) *PEs motivating stressed OVs in Brazilian Portuguese*

i	$(\underline{\mathrm{I}})$
u	$(\underline{\mathrm{U}})$
a	$(\underline{\mathrm{A}})$
e	$(\mathrm{A} \cdot \underline{\mathrm{I}})$
ɛ	$(\mathrm{I} \cdot \underline{\mathrm{A}})$
o	$(\mathrm{A} \cdot \underline{\mathrm{U}})$
ɔ	$(\mathrm{U} \cdot \underline{\mathrm{A}})$

In addition to this, there are five stressed NVs in BP:[30]

[29]I also neglect here which of the two PE constraints, *i.e.* the phonologically violable LC 'Operators must be licensed'—as in (34)—or the inviolable 'PEs must be headed', is the more accurate one in BP.

[30]My informant (Angelica Sampaio) transcribes 'ã' as 'ɐ̃'; even though such a transcription might be phonetically more accurate, their is no phonological, *i.e.* cognitive, reason based on which the distinction between ã and ɐ̃ could be viewed as phonologically relevant in BP.

(36) *Stressed NVs in Brazilian Portuguese*

ĩ	⟨pinga⟩	pĩ̱gɐ	'type of alcoholic beverage'
ũ	⟨junta⟩	ʒũ̱tɐ	'joint *(anatomy)*'
ã	⟨banda⟩	bã̱dɐ	'band'
ẽ	⟨crença⟩	krẽ̱sɐ	'belief'
õ	⟨conto⟩	kõ̱tu	'tale'

Importantly, a tense-lax distinction as it is observable for stressed mid OVs is not possible for stressed mid NVs. Let us now assume in agreement with the RET that fused NVs, *i.e.* nuclear PEs which contain L and other elements all of which are fused lexically, have an L-head as part of their internal representation. This correctly predicts five stressed NVs (table 6.1, b., p. 258). Since there is no underlying NV in BP which contains no aperture element—so there is no (Ḻ)—we need to postulate a GC which predicts this. I propose 'L must license operators' for this purpose.[31] I repeat the stressed OVs in (a.) and give the seven stressed NVs incorrectly predicted by the assumption of an L-*operator* for fused NVs in (c.):

Table 6.1: Stressed vowels in Brazilian Portuguese

a. *OVs*		b. *NVs*		c. *Ill-formed set of BP NVs*	
i	(I̱)	ĩ	(I · Ḻ)	ĩ	(L · I̱)
u	(U̱)	ũ	(U · Ḻ)	ũ	(L · U̱)
a	(A̱)	ã	(A · Ḻ)	ã	(L · A̱)
e	(A · I̱)			ẽ	(L · A · I̱)
		ẽ	(A · I · Ḻ)		
ɛ	(I · A̱)			ɛ̃	(L · I · A̱)
o	(A · U̱)			õ	(L · A · U̱)
		õ	(A · U · Ḻ)		
ɔ	(U · A̱)			ɔ̃	(L · U · A̱)

As table 6.1 shows, the assumption of fused NVs containing an L-head predicts the correct set of stressed OVs and NVs. Note that in (b.), it is arbitrary in relation to the BP NVs whether one chooses the symbol for the tense or lax mid vowels, *i.e.* ẽ õ or ɛ̃ ɔ̃. The reason for this is that in the analysis motivating the well-formed sets in table 6.1, the phonetic distinction 'tense *versus* lax' is not necessarily phonologically relevant. In the explanation put forward here, the set of NVs is not derived from the set of OVs; so there is simply no distinction between 'tense' and 'lax' for BP NVs.[32]

[31]This means that the GCs for the L- and, possibly, the H-element may differ from the ones for the aperture elements presented in chapter 4.

[32]Phonlogically motivated contrasts which are circumscibable in phonetic terms as tense-lax oppo-

To predict that languages have the option to allow different kinds of nuclear expressions involving an L-element—resulting in low tone and/or nasality—we may have to stipulate another parameter (or a whole series of them)[33] with the default setting OFF: L-heads are fusable with nuclear material (OFF). This way, many languages simply have no fused NVs (and/or other vowels linked to L in some way). Note however that we could set up our system of GCs (default settings) in a way such the LC 'L must license operators' can only be switched ON if L may fuse with other elements attached to a nucleus at all. So instead of proposing 'L may fuse with elements when attached to a nucleus' and, additionally, a constraint restricting such a fusion further, we can simply assume a restriction on L, *e.g.* 'L must license operators', which presumes that L may fuse with other nuclear melodic material.

Finally I have to point out that the analysis of BP stressed vowels presumes that GCs may be specific to stressed or unstressed vowels. I do not see this as problematic because it is a cross-linguistically common phenomenon that unstressed vowels have a reduced licensing potential (*cf.* Harris [203]). This picture becomes even more complicated by the fact that in many languages there is not only a distinction between stressed and unstressed vowels with repect to the neutralisation of oppositions but, in addition, pre- and posttonic vowels may exhibit different restrictions. BP is a good example of this phenomenon (*cf.* Sampaio [428]): while pretonic nuclei may only be underlying i u a ɛ ɔ[34] and ĩ ũ ã ẽ õ, posttonically, there are no underlying NVs (*i.e.* NVs which are not derived by binary leftward nasal spreading from a following onset dominating m or n and which do not involve the analytical (closed class) third plural suffix). Underlying posttonic OVs which are non-final are, like pretonic OVs, i u a ɛ ɔ. Domain-final OVs are i u a. Apparently, we need different GCs catering for these distinctions in relation to stressed, pre- and post-tonic and domain-final position.

My analysis of these fact is as follows: As stated above, in BP, I and U may not

sitions are in the RET explained by headedness distinctions. For example, Walker [485] and, subsequently, Cobb [110] analyse tense-lax contrasts in ATR-harmony languages as headed-headless constrasts. Similarly, for languages which differentiate short lax from long tense vowels, such oppositions are equally viewed as headless (short)–headed (long) oppositions (for English, *cf.* Kaye [262], for German, Ploch [379]). Tense-lax differentiations which are restricted to the mid vowels are motivated in the RET by contrasts in relation to the nature of the element in head position (*e.g.* (a.) in table 6.1 I-headed e *versus* A-headed ɛ).

[33] It is in my view empirically fine to establish a set of assumptions by continuous abstraction from many towards fewer stipulations as opposed to the top-down approach which would postulate a 'grand' idea whose more numerous counter-examples have to dealt with afterwards. Both approaches differ from the PH which is simply unfalsifiable.

[34] According to Sampaio [*ibid.*], pretonic lexical ɛ ɔ are in certain environments turned into tense vowels (e o); *cf.* Cobb [110] for analysis of this process.

fuse, which can be said independently of the 'stress-related' phonological environment. 'Elements may fuse' is only switched ON for domain-internal (*i.e.* non-final) and all stressed nuclei. Since 'L must license operators' this also predicts why there are no domain-final unstressed NVs in BP. Therefore the domain-final vowels are i ($\underline{\text{I}}$), u ($\underline{\text{U}}$) and a ($\underline{\text{A}}$). The fact that domain-final vowels sound lax and centralised is not phonologically relevant without evidence for such a view. Note also that, as I will argue in the following, non-final unstressed vowels are headed. The idea that the distinction between stressed vowels (which are stressed and thus 'strong' and therefore headed) and unstressed vowels (which are weak, reduced and headless) is an entirely intuitive and not necessarily empirical one.

In addition to this, I propose that non-stressed nuclei are headed and are restricted by the GC 'A must not be licensed'. This explains why for unstressed (non-final) nuclei, there is no tense-lax distinction for mid vowels. One might want to put forward that unstressed vowels are headless because this assumption equally results in a system with no tense-lax distinction for mid vowels: i (I), u (U), a (A), e/ɛ (A · I), o/ɔ (A · U). However, since NVs are allowed in such (unstressed) contexts and since BPs are headed (*i.e.* L-headed), one would have to add a further stipulation exempting NVs from the constraint that unstressed vowels must be headless. I maintain that all Portuguese vowels are headed and that unstressed vowels are further restricted by 'A must not be licensed', a constraint which is vacuous in the case of domain-final vowels which do not allow any fusion. So we get the non–domain-final lexical unstressed PEs: i ($\underline{\text{I}}$), u ($\underline{\text{U}}$), a ($\underline{\text{A}}$), ɛ (I · $\underline{\text{A}}$), ɔ (U · $\underline{\text{A}}$), and ĩ (I · $\underline{\text{L}}$), ũ (U · $\underline{\text{L}}$), ã (A · $\underline{\text{L}}$), ẽ (A · I · $\underline{\text{L}}$), õ (A · U · $\underline{\text{L}}$). We see that for BP vowels, it is not quite accurate to claim that the distinction 'pre- *versus* prosttonic' is relevant for unstressed vowels; rather, it matters first and foremost whether a nucleus is stressed and, if it is unstressed, whether it is domain-final.

The following list summarises the GCs which are switched ON for BP vowels:

(37) *GCs for BP vowels*

1. Elements may fuse. (ON for stressed and unstressed non-final nuclei)

2. Operators must be licensed.

3. L must license operators.

4. A must not be licensed. (ON for unstressed nuclei)

Finally, I have shown in this section that based on the system of contrasts attested in BP, it is possible to come up with a small set of assumptions which is (mostly) backed up cross-linguistically by independent evidence and which predicts the apparent phonotactic constraints for OVs and NVs. To my knowledge, it is not possible to do this by looking at the phonetic signal.[35] The weakest point of my analysis is that the LC-type which I proposed for L, *i.e.* 'Element X must license operators' is not supported by independent evidence, which is not particularly surprising because no-one other than myself has, to my knowledge, worked on GCs/LCs for NVs. A closer inspection of this constraint will form an important part of my future investigation of this topic.

Conclusion

In this chapter, I have provided additional evidence for my claim that it must established based on the system of contrasts and/or the phonological processes a language employs which phonetic details are phonologically relevant. In other words, as pointed out again and again throughout this thesis, measurements of phonetically defined properties may merely be a source of ideas which have to be tested phonologically but do motivate the observed contrasts or processes. More specifically, I have shown following my argumentation against a phonetic explanation of (phonetically) 'nasality-induced vowel height shifts' in chapter 3 that there is a cognitive explanation possible for the widely discussed affinity between low/backness and nasality in French. In addition to that, I have looked at BP vowels, particularly stressed vowels, and explained the neutralisation of tense-lax distinctions (attested for stressed OVs) in the case of stressed NVs and observable for all unstressed vowels.

All in all, it can be said that a non-phonetically motivated cognitive phonological explanation does not share the unfalsifiability inherent in phonetic approaches. Furthermore, the cognitive analysis presented here leads to the recognition of a phenomenon which has not been observed before, which makes it particularly interesting: acoustic cue overlap. As Mahu ã can be motivated by two different types of phonological representations (*cf.* chapter 4), QF ã is motivated by yet another. This is in line with Kaye's view ([253, 262]) that a large part of the phonetic signal is

[35]For example, Redenbarger [404] investigates phonetically and 'phonologically' defined vowel height in Portuguese. Of course, the phonological height he looks at is already phonetically motivated. On the basis of my arguments against the PH presented in part 1 of this thesis, it is, in my view, not possible to use his findings as part of a phonological, *i.e.* cognitive, analysis of Portuguese vowels.

merely 'packaging' and phonologically uninteresting. My analysis shows that tense-lax distinctions for vowels do, defined in this phonetic way, not provide the relevant cue necessary to analyse such a constrast phonologically. I could only establish independently of phonetics that tense-lax distinctions for stressed oral mid vowels in BP are based on differentiations in relation to what aperture element (A or I/U) forms the head of a PE and not, as in English (Kaye [262]) or German (Ploch [382]), on headed-headless constrasts. Similarly, the neutralisation of tense-lax distinctions for unstressed OVs and all NVs in BP is caused by different phonological means: by the LC 'A must not be licensed' in the case of unstressed OVs and by the assumption of fused NVs exhibiting an L-head for NVs. Clearly, phonological nasality is not a phonetically motivated phenomenon.

Chapter 7

Consonants as Nasalisation Targets

In this chapter, I will investigate nasal assimilation phenomena which have consonants, *i.e.* onsets, as targets.[1] In agreement with the argumentation presented in earlier chapters of this thesis, I will show that without a closer look at the internal representation of the target consonants involved, there is no sensible explanation of the phenomena in question. Furthermore, it will become clear that only a cognitive theory, not a phonetically grounded one, can explain what consonants are opaque or transparent to nasal assimilation. Most importantly, I will argue that, as in the case of vowel systems (*cf.* chapter 4), one of the purposes of phonology is to help the acquirer disambiguate acoustic cue overlap apparent in consonants, *i.e.* to decide what the precise definition (*i.e.* internal representation), of the consonants (which make up the consonant system of a language) looks like. Without the phonology, such ambiguous systems would be unacquirable. Since this is a vast topic, I will only illustrate my findings on the basis of an investigation of N-prefixes or, as in the case of Indonesian, of a prefix containing N (*i.e.* məN-).

7.1 Korean

I will start my discussion of nasalisation of consonantal targets by providing an analysis of nasality-induced voicing in Korean. I have already introduced the relevant type of data in chapter 5, particularly in section 5.2.1.

In (8) in chapter 5 (p. 198), I have pointed out that one can observe postnasal voicing of neutral—but not of tense or aspirated—stops. In (1), I give an abbreviated version of the data presented there:

[1] Since nasal stops in coda position in versions of GP with rhymes and branching constituents have been reanalysed as (ʔ · L) dominated by an onset position, my analysis of consonantal targets does not include nasal stops linked to onset licensees which are not onsets themselves.

(1) *Nasality-induced voicing of neutral stops in Korean*

a. Neutral obstruent → voiced obstruent / nasal stop _

s°oːŋgot°	'gimlet'
t°oŋbæ	'dividing'
ʧ°aŋdori	'hammer'
s°iːmburɨm	'errand'
namʤit°	'over'
ʧ°amgi	'to sink'

b. Postnasal tensed or aspirated obstruents

p°ənʧ'ək°	'sparkling'
t°oːŋʧʰimi	'pickled cabbage'

We can see that in Korean, only neutral stops, *i.e.* those not containing an H-element,[2] are affected by postnasal voicing. What is interesting about the Korean data is that here, as opposed to many other languages which display postnasal voicing, the nasal stops triggering this effect do *not* have to be homorganic to the voicing target. So in Korean, N containing an aperture element lexically can be inter-onset licensed. I propose that there is a head-final relationship between an onset (O_2) and a preceding N in O_1—which is licensed by an empty nucleus (N_1) which is p-licensed by the inter-onset licensing relationship holding between O_2 and O_1.[3] Also remember that we have independent evidence for the assumption that in Korean, there is inter-onset licensing and that such a relationship must be head-final (*cf.* chapter 5). The difference between NC-sequences containing a neutral stop and those with a tensed or aspirated stop is that the inter-onset relationship is in the case of voiced stops additionally expressed by an L-element which spreads from N to C in a way such that it is operator in N—motivating a nasal stop—while it is head of the following voiced stop.

We have seen L switching its lexically assigned H/O role when spreading before, *i.e.* in our discussion of the first singular prefix in Bemba in relation to Meinhof's Law (chapter 5). The reason that tensed and aspirated stops cannot take part in this kind of L-sharing is the aforementioned L/H-compatibility; *e.g.* p' $(ʔ \cdot \mathrm{U} \cdot \underline{\mathrm{H}})$ and p^h $(ʔ \cdot \mathrm{U})(\underline{\mathrm{H}})$ both contain an H-element, a neutral stop, *e.g.* p° $(ʔ \cdot \mathrm{U})$—which becomes b $(ʔ \cdot \mathrm{U} \cdot \underline{\mathrm{L}})$ when nasalised—, does not. So an H in the target consonant prevents L from

[2]Remember that there are no lexical PEs containing an L-head in Korean.

[3]O_1, O_2, *etc.*, are variables, not fixed positions in a domain. Actually, the leftmost nucleus of a Korean domain can never be p-licensed.

attaching to C. As we will see below, in order to avoid L/H-incompatibility resulting from attempted L-fusion with a PE containing an H-head, Zoque and Indonesian do not choose the strategy not to fuse L with the stem-initial onset—which is the option Korean (analytical morphology) and Bemba take—but to go ahead with fusing L and simultaneously delink a lexically asssigned target H-head.

Of further interest is here that without L-spreading some of the neutral stops (the velar ones) would be less complex than the nasal stops which they are supposedly governing. For example, in ʧ°amgi, lexical k° (ʔ), the governor, would not contain either an L- or H-head and would also be less complex than its governee, underlying m (ʔ · U · L). Such a situation would result in the intervening empty nucleus being unlicensed and audible. Note that in the (neo-)RET, the complexity constraint on government only applies if the governor does not contain an L-head or H. PEs containing an L-head or, if no other constraint like the one in (19) in chapter 5 does not prevent this, an H-element are always good governors. So we can say that in langugages like Korean, in which N of an inter-onset NC-sequence may contain an aperture element because of which N will be in many cases be more complex than governing C, L-spreading with L forming the head of C can be seen as a phenomenon which makes inter-onset licensing—and thus p-licensing of the intervening empty nucleus—possible. Without L-assimilation, Korean N (in NC-sequences) could either not contain an aperture element lexically or the empty nucleus following N would have to be phonetically realised. [4]

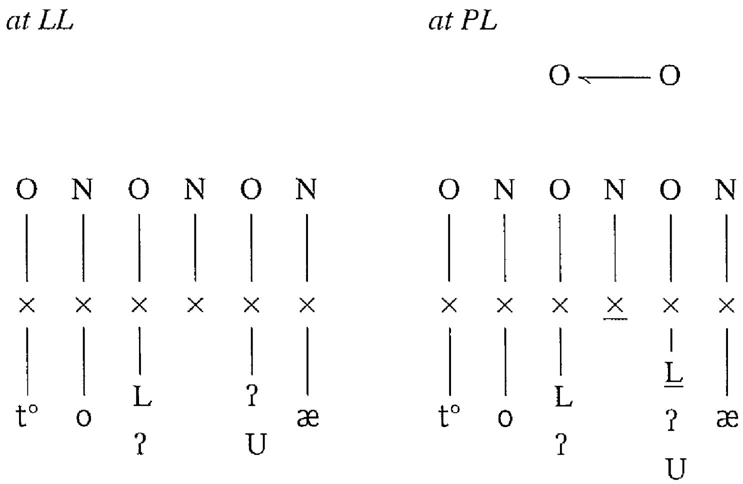
The following representations illustrate this:[5]

[4]For this analysis to work, I assume that the ʔ-element is never taken into account by the complexity counter. As pointed out in previous chapters, ʔ seems to be the melodic metaphor the (neo-)RET uses to explain a structural phenomenon. One of the next steps in improving the neo-RET is, it seems, to substitute the ʔ-element with structure. Note however that Jensen's [238] attempt to do so accounts only for a relatively small amount of data.

[5]As elsewhere, non-branching rhymes as in (2) have been omitted.

(2) *Nasality-induced voicing in Korean (structures)*

a. t°oŋbæ



Another interesting observation that can be made in this context is that in (2), L spreads at PL from left to right as part of a licensing relationship which exhibits the reverse directionality (right to left). What we have to ask ourselves is whether this is possible. In GP, when spreading was analysed as a manifestation of licensing, *e.g.* by Charette & Göksel [86, 87] for a number of Turkic languages, it was naturally assumed that the spreading element—in Turkic languages I and/or U—would spread from licenser to licensee. Since 'harmonisable' nuclei are more constrained in the lexicon in relation to what PEs are licensed in such a position, it was regarded as theoretically well-formed to assume that elements spread from licenser to licensee. It follows that my analysis goes against this tradition. However, as I have shown in [381], data from Augsburg Swabian, Bermeo Basque and Québec French ('QF') suggest that Brockhaus's [67] view that licensing is the motor driving phonology is flawed. In the same article I also discuss in what way Charette & Göksel's ('CG') analysis of Turkic harmony phenomena as licensing processes is not without problems. As a consequence, the view that the spreading of elements must proceed from licenser to licensee is not at all firmly established. I conclude that my analysis of L-spreading going against the licensing direction does not contain an argument against it.

For convenience's sake, let me sum up the relevant terminological changes and theoretical implications.[6] I have shown in [381] that GP predicts processes which are not licensing processes. In short, the Licensing Principle, proposed by Kaye [254],[7]

[6]The rest of this section (7.1) is taken from [381].

[7]The Licensing Principle states: "All phonological positions save one must be licensed within a

does in no way imply that all phonotactic constraints or all phonological processes must be motivated by and be manifestations of the Licensing Principle. So henceforth, I will only refer to a process as licensing process if it is an instantiation of the universal constraint stipulated in GP by the Licensing Principle that all phonological positions save one must be licensed within a domain. In other words, a licensing process marks and points out the head of the domain, and not every process has this property. Since in GP, phonology is (partly) motivated by a parsing function (*cf.* section 4.1.1), let us say that all phonological processes are *parsing processes*, to pick a term, while only some of them are *licensing processes*.[8] As each type of licensing process (proper government, h-licensing, *etc.*) is, of course, a licensing process, all processes, including licensing processes, are parsing processes. Similarly, as each licensing process reflects an asymmetrical relationship between *licenser* and *licensee*, each parsing process reflects an asymmetrical relation between *parser* and *parsee*. That is to say that all processes in human languages operate from parser to parsee independently of whether the process in question instantiates a a licensing relationship.

Furthermore, phonology parses by establishing parsing relationships between parsers and parsees of different types, *e.g.* spreading of an element, head-alignment, some type of licensing, *etc.*, some of which may be universal, some language-specific. Note that any supporter of the PH would have to stipulate this kind of parametric variation. The cognitive and the phonetic approach are equal in this. However, the PH adds that either all parametric variation or some of it is phonetically motivated, and it is this claim which is unfalsifiable. In other words, the PH adds absolutely nothing to an explanation of phonological phenomena but provides a comfortable way for researchers who cannot bear to be proven wrong to make sure that their assumptions will never embarrass them.

Assuming, as GP does, that all phonological processes are subject to adjacency, parser and parsee must be adjacent too, or, rather, we can express the universal adjacency condition in terms of parsers and parsees:

(3) *The Adjacency Condition*

Parser and parsee must be adjacent at the relevant projection.

This enables me to define a parser-parsee relationship as an asymmetrical relationship

domain. The unlicensed position is the head of the domain" [254, p. 306].

[8] In [381], before I proposed the Acquistional Hypothesis (*cf.* [383] and this thesis, section 4.1.1), the notion of parsing was not acquisitionally motivated—at least not as specifically as in my present approach.

subject to adjacency. This definition encompasses precisely what it is supposed to, *i.e.* all phonological processes and even lexical phonotactic constraints—since parsing is not defined as an obligatorily phonological (derived) phenomenon—. nothing more, nothing less. Within this relationship, the parsee is the recessive member or the dependent. A parser, then, is a point on one of the theoretically possible projections in GP which parses its parsee via some asymmetrical relationship, *e.g.* a (trans)constituent governor parses its parsee, the (trans)constituent governee, via (lexically established) (trans)constituent government, which is a type of licensing; an element spreads from parser to parsee. In general, a parser has an effect on the parsee in a way such that the parsee is more restricted in its passive prosodic and/or melodic parsing (*e.g.* licensing) potential than its parser.

Let me provide examples. In Turkish, we can observe that I- and U-spreading only occurs in some domains, not in others. So there are many domains of Arabic, Persian, French origin which are not vowel-harmonic (*e.g.* hukuki 'law-related', from Arabic; metres 'maitresse', from French). Vowel harmony would therefore only mark the head of the domain, *i.e.* the left-most unlicensed nucleus, in harmonic words. However, there is one phonological phenomenon which in both harmonic and non-harmonic words singles out one nucleus: (primary) stress. Therefore I propose that Turkish vowel harmony is a parsing (but not a licensing) process, while stress is the manifestation of licensing relationships.

Furthermore, in Turkish, some words ending in palatal l^j exhibit an I-element dominated by the (unlicensed) nucleus of certain suffixes where I can only have spread from l^j (in onset position) to the following nucleus, *e.g.* in usul^{j}y̲ (third singular possessive of usul^j 'method', with y containing both I and U (*cf.* chapter 4).[9] We can explain the U-element in the suffix (-y) as derived via U-spreading at the nuclear projection. However, the I in y must have come from l^j, *i.e.* I spreads from an onset to the following nucleus which licenses that onset. Apparently, spreading is not necessarily a licensing process and may even go against the directionality of a licensing relationship.

In Japanese, on the other hand, the distribution of high pitch singles out the head of the domain, *i.e.* the (lexically or phonologically) primarily stressed nucleus of a domain (*cf.* Yoshida [513]). This points to an analysis of Japanese pitch accent in terms of licensing.

[9]Remember that I am employing IPA symbols here, so y does not represent a 'palatal glide', to use a phonologically irrelevant term, but a 'rounded high front vowel'.

Let me point out here that there is a difference in quality between the asymmetry of the relationship between parser and parsee and its subtype between licenser and licensee. This difference does not have to stipulated, it simply follows from a version of GP which does not subscribe to Brockhaus's limited view. The asymmetry between licenser and licensee must be phenomenon-*independent*,[10] *i.e.* in a string of two points a and b adjacent on some projection, if a licenses b at LL or via ϕ at PL, then b cannot license a on any level or projection, *e.g.* by some other process. If the asymmetry between licenser and licensee were phenomenon-dependent, more than one domain-heads would be possible. Since the Licensing Principle precludes this, it predicts the asymmetry between licenser and licensee to be phenomenon-independent (and merely domain-dependent). Furthermore, since some licensing relationships are established in the lexicon, *e.g.* government relationships, the asymmetry between licenser and licensee would by definition be phenomenon-independent at LL.

On the other hand, the asymmetry between a parser and a parsee which are not in a licensing relationship can be definition only be phenomenon-*dependent*. To restrict all parsing relationships to phenomenon-independency would firstly require an additional assumption stipulating this and would secondly put an unmotivated limit on the nature of possible parsing relationships. Therefore, I do not propose that the parser-parsee relation is universally, or, in a given language, phenomenon-independent. I consequently, predict phonotactic constraints in which in a string 'a, b' (adjacent on some projection) in a given domain, a can be the parser of b in one phenomenon P_1, while b can be the parser of a in some phenomenon other than P_1.

In relation to our discussion of postnasal voicing of neutral stops in Korean, this means that an NC-sequence exhibits two simultaneous parsing relationships with opposite directionalities: a head-final licensing relationship from C to N, which is also a parsing relationship (thus excluding any licensing relation from N to C) and a head-initial parsing relationship from N to C expressed by L-spreading. Note that there is evidence independent of postnasal voicing in favour of the proposal of head-final (but not head-initial) inter-onset relationships in Korean. This also means that in other languages in which we find NC-sequences, N can never license C. Also, we have seen such simultaneous parsing relationships with opposite directionalities before in our discussion of the Bemba nasal prefix (*cf.* section 5.2.4.2): the stem-initial

[10]By 'phenomenon', I mean 'process' (a dynamic constraint) or a static phonotactic constraint (holding at LL).

onset inter-onset licenses the onset dominating N of the nasal prefix, while N moves (if possible) to the stem-initial onset as a parsing, not a licensing, phenomenon.

Of course, none of these differences between licensing and parsing and its different predictions with respect to the co-occurrence of phonological phenomena can be motivated phonetically. Supporters of the PH would have to neglect to apply the PH in such cases. Furthermore, the different directionalities involved can also not be predicted phonetically. Note that this has to be stipulated in a cognitive model too. However, such a cognitive conjecture would have to be made with or without the PH, which points to the main problem of the PH: it never contributes anything to an explanation. While a cognitive approach does not pretend it can explain phenomena it cannot explain, the PH does suffer in this manner and is therefore self-delusional. This does not mean that a cognitive approach cannot explain phonological phenomena. Each explanation is a set of assumptions, *i.e.* stipulations, no matter whether or not the assumptions in question are phonetically grounded. However, following Popper's proposal of falsifiability as demarcation criterion between empiricism and non-empiricism—and following Ockham's Razor—we have to let theories compete in order to be empirical because falsifiability is a relative notion. In this view, one of the best arguments against any assumption is the situation when it can be shown that it is not necessary. In all the examples I have seen—and I have discussed many of them in part 1 of this thesis—the PH is never, and I repeat, never, necessary. Since the presence of a phonetic property does not imply its phonological relevance and since this question always has to be decided independently of the very phonetics which supposedly motivate phonology, I can maintain that the PH is totally unnecessary.

Finally, let us briefly look at nasalisation phenomena in Korean which go across a domain-boundary, *i.e.* where analytical morphology is involved. Consider the following examples (taken from Heo [212, pp. 142ff.]):

(4) *Prenasal nasalisation of obstruents in Korean*

ʧ°i<u>p°</u>+man	ʧ°i<u>m</u>+man	'house + only'
o<u>s°</u>+mank^{h}ɨm	o<u>n</u>+mank^{h}ɨm	'clothes + as'
k'ə<u>k'</u>+ni	k'ə<u>ŋ</u>+ni	'to pick (interrogative)'
p°uə<u>k^h</u>+mat°a	p°uə<u>ŋ</u>+mat°a	'kitchen + every'

Here we see the stem-final onset inter-onset licensing the suffix-initial nasal stop in combination with a parsing relationship going the opposite direction. What is different from the Bemba N-prefix (*cf.* chapter 5) and from domain-internal inter-onset licensing relationships in Korean is—apart from reversed directionalities—that leftward-spreading L can knock out an H that is lexically assigned to the target onset. This way, if analytical morphology is involved, all stem-final onsets containing H can be nasalised by a suffix-initial onset dominating an L-operator. In section 7.3, I will discuss a similar case in Zoque. As I will show, the difference between Korean and Zoque in relation to L knocking out H is that in Korean, any H can be knocked out (head or operator), while in Zoque, only H-heads but not H-operators can be pushed out. So in Zoque, only voiceless stops (containing an H-head) but not voiceless fricatives (containing an H-operator) can be nasalised.

In the following, I will extend my analysis of nasality-induced voicing to examples from other languages.

7.2 Ecuador Quichua

As discussed in section 5.2.1 (*cf.* (1), p. 194), in Ecuador Quichua, suffix-initial voiceless stops turn into voiced ones after stem-final nasals; such nasals are homorganic to the following voiced stop. I repeat a shortened version of the data presented in chapter 5:

(5) *Nasality-induced voicing in Ecuador Quichua*

ʧilis-pa	'streamless region's'	hatum-ba	'the big one's'
punʤa-pi	'in the daytime'	atam-bi	'on the frog'
puru-ta	'gourd'	atan-da	'frog'
lumu-ʧu	'manioc?'	tijan-ʤu	'is there?'

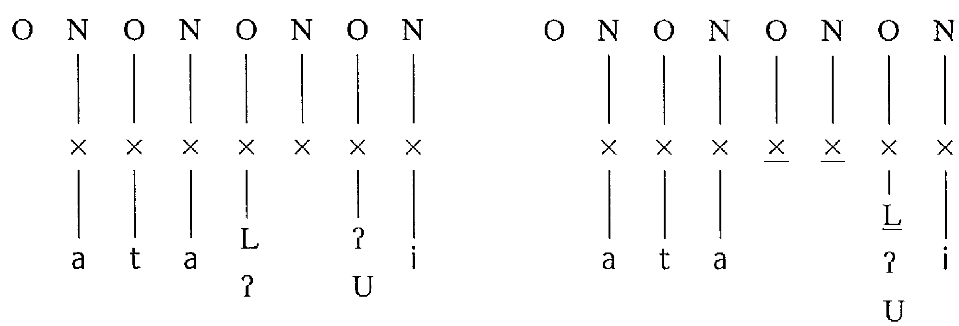
Based on these data, it is impossible to decide whether the suffixes in (5) are analytical or non-analytical (*cf.* my introduction to the GP in chapter 4). This would have to be decided—not by studying the phonetics, of course—but by an analysis of the cognitive system involved. For example, in (5), we find ʧilis-pa containing the sequence sp. If it were the case that in Ecuador Quichua, there are no sp-sequences unless morphology is involved, it would follow that s and p in sp belong to different domains in the case of the -pa-suffix. However, clusters of 's or ʃ preceding stops' are well-formed [356, pp. 68, 70]. In my analysis of the Quichua data, I will assume the suffixes in (5) to be non-analytic, *i.e.* the phonology does not recognise that morphology is involved. I will also stipulate that 'voiceless' stops in Quichua are neutral stops and contain no H-element. Note the reason for this is not because otherwise my L/H-incompatibility based analysis would not work. The wide-spread

L/H-incompatibility is not universal.[11] As long as there is no evidence for the assumption of an H-element for 'voiceless' stops in Ecuador Quichua, I will not assume it.

Let us say that in Ecuador Quichua, there is, as in Korean and Bemba, a head-final relationship—from voiceless stops to N and, as in Bemba but not as in Korean, an additional head-initial parsing relationship which is expressed via movement of an L-operator (lexically dominated by the source onset) to C (the following onset), fusing with C as head, which results in a voiced stop at PL. Remember that we know from Bemba and our analysis of Meinhof's Law that N does not spread but moves. So the relevant constituent structures for atambi at LL and PL are:

(6) *Ecuador Quichua* atam-bi

a. *at LL* b. *at PL*



7.3 Zoque

In Zoque, there is a nasal prefix (first singular possessive) which is homorganic with the following stem-initial consonant. Stem-initial voiceless stops turn into voiced ones. N is n before w j h and is lost before ʔ f s ʃ m n ɲ l r (*cf.* (3) in chapter 5). The relevant data can be summed up as follows—C refers to a stem-initial onset, N + C to the result of having added the first singular N-prefix to the stem-initial onset:

[11] The difference between type A and type B nasal harmonies (*cf.* chapter 2) is in my view motivated by a language-specific distinction in relation to the active or passive status of the L/H-incompatibility constraint (*cf.* [376]).

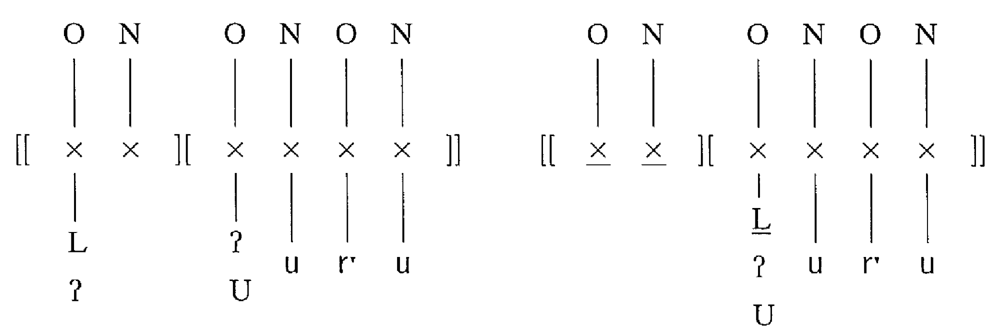
(7) *Nasality-induced voicing in Zoque*

C	N + C	C	N + C
p	mb	ʔ	ʔ
b	mb	f	f
t	nd	s	s
d	nd	ʃ	ʃ
ʧ	ɲʤ	m	m
k	ŋg	n	n
g	ŋg	ɲ	ɲ
w	nw	l	l
j	nj	r	r
h	nh		

We see that in the case of stem-initial voiced stops—which I analyse as neutral stops (*e.g.* b (ʔ · U))—C licenses N from right to left and, at the same time, there is, as in Bemba, a left-to-right parsing relationship from N to C which expresses itself (where possible) via the melodic material of N delinking from its lexically assigned onset, moving to the stem-initial onset and fusing with the elements present there, resulting in a sequence of an empty onset (which dominates N at LL), followed by a p-licensed empty nucleus, which, in turn, precedes an L-headed prenasalised voiced stop. Since this prefixation phenomenon creates structures which are not part of the Zoque lexicon, I will assume the N-prefix to be analytical (as in Bemba):[12]

(8) *Zoque* m-burˑu 'my burro' (*from* burˑu)

a. *at LL* b. *at PL*



Note that we cannot claim that in Zoque, mb (or any other prenasalised voiced stop) consists, at least at PL, of m (ʔ · U) dominated by an onset followed by (an empty nucleus and then by) another onset dominating lexically L-headed b (ʔ · U · <u>L</u>) even

[12]Zoque contrasts l r rˑ; rˑ is used in certain Spanish loanwoards.

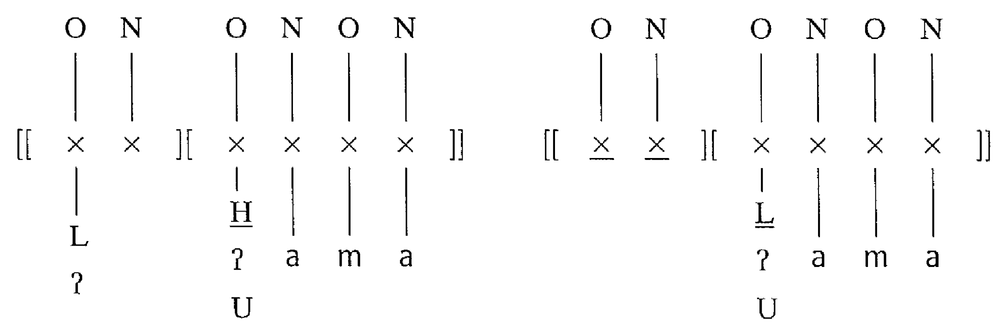
though, this way, we could account for voiceless stops as neutral stops (*e.g.* p (ʔ · U)). Within such an analysis, we would not have to drop the L/H-incompatibility constraint (*cf.* section 5.2.3), which is what we have to do if we assume that Zoque voiceless stops—which *are* affected by postnasal voicing—contain an H-head.[13] However, this would not be in line with the analysis of the Bemba N-prefix which showed that, at PL, mb as the result of N-prefixation is motivated by one PE dominated by one onset point (*cf.* (24b.)). So as in the case of the Korean analytical suffixes discussed in section 7.1, L can knock out H. The difference between Korean and Zoque is that, while in Korean any H-element can be knocked out, in Zoque only H-heads can be pushed out by L, *i.e.* in Korean all stops (neutral—without H—, tensed—with fused H-head—, and aspirated ones—with unfused H-head) and fricatives—with H-operator—can be substituted by L in analytical morphology, in Zoque only voiceless stops—with H-head—but not (voiceless) fricatives—with H-operator—can be fused with L (with H delinking as part of this fusion operation).[14]

I have already provided the constituent structure of an example with a stem-initial lexical voiced stop in (8). However, Zoque is different from Bemba in that in Zoque, N must be inter-onset licensed otherwise N is delinked, while in Bemba it will simply not move but stay attached, resulting in Bemba m-futa 'I pay' (from -futa) next to Zoque faha 'my belt' (from faha).

In the following I give the representations for a stem-initial voiceless stop (9) or (voiceless) fricative (10):

(9) *Zoque* m-bama 'my clothing' (*from* pama)

a. *at LL* b. *at PL*

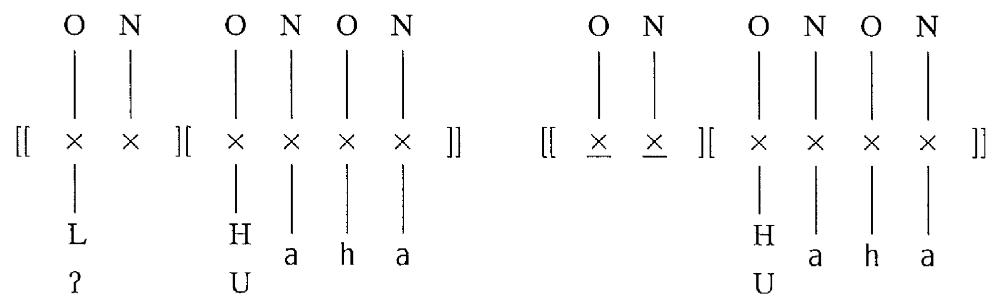


[13]Remember that, even though we do not know why, there are no PEs in the (neo-)RET which contain H and ʔ which may contain H as operator.

[14]Even though this would be beyond the scope of this chapter, the reason for this difference between Korean and Zoque analytical morphology might be that Zoque, like Bemba but unlike Korean, has the constraint on L-spreading in relation to the H/O role L may occupy in the target that L must become head.

(10) *Zoque* faha 'my belt' (*from* faha)

a. *at LL* b. *at PL*



In order for this analysis to work we have to assume in line with the (neo-)RET that in Zoque—as in other languages (*e.g.* Indonesian, below)—only PEs containing H-heads, not H-operators, or those with an L-head can govern N at PL. This way fricatives cannot license N, and since L cannot move into a fricative (because of L/H-incompatibility and because in Zoque L trying to attach to the target PE as head can only knock out an H-head but not an H-operator), N before a (voiceless) fricative is completely lost in Zoque. Since, as I have pointed out in my analysis of Bemba, ʔ-spreading—and let us say, ʔ-delinking too—is parasitic on L-spreading—or -delinking—stem-initial fricatives are not turned at PL into voiceless stops via rightward ʔ-spreading.[15] So since N must be licensed but fails to be in the case of stem-initial fricatives, faha simply has no audible prefix. Remember that an important reason for the assumption of empty prefixes as in (10b.) is that empty prefixes (like the dissimilated version of the AS past participle prefix) correspond to m in the case of Meinhof's Law, *i.e.* to a *filled* onset. My comparative analysis of Meinhof's and Dahl's Law argues that the complete perceptual disappearance of the past participle prefix in AS is a radical case of delinking, and delinking does not imply loss of structure. Similarly, in Zoque, the delinking of N does not presume that the structure that we see in all cases where no delinking occurs is deleted too.

The constraint (*i.e.* that only H-heads can govern N at PL) is predicted not to apply in the case of stem-initial voiced (neutral) stops (*e.g.* b (ʔ)) (*cf.* (8)): since the melodic material of N can fuse with a neutral stop, the resulting prenasalised voiced stop (linked to one stem-initial onset point) can govern the prefixal onset point (empty at PL) since the restriction that, at PL, N in the prefix must be licensed by an H-head is vacuous; in the case of voiced (and voiceless) stops at PL, there is

[15]I do not know whether ʔ-spreading, -movement and/or -delinking is universally or parametrically parasitic on L.

simply no N attached to the prefixal onset point at PL. Let us also say that the inter-onset licensing relationship that licenses onsets of empty syllables is simply subject to the Complexity Condition (Harris, *cf.* above). This means, that any filled onset can inter-onset license such an onset.

Furthermore, N (ʔ · L) cannot fuse with a stem-initial nasal stop because such a nasal stop already contains all the elements which could be gained from N. Since this means that at PL, N (not being able to move) would still be attached to the onset of the prefix and since such an onset would have to be licensed by an H-head—which a stem-initial nasal stop does not contain—, prefixal N is delinked in Zoque when preceding a nasal stop. Note that this does not happen in Bemba (nnaka) because Bemba does not employ the must-contain-H̲ constraint on the licensers of N-licensees.

In order to predict why N is lost before l r, let me point to the fact that in languages with NC-sequences, l r can never license N, so there are no Nl- or Nr-clusters in such languages. However, to explain why L does not fuse with r (A)/(I) or l (ʔ · A)/(U),[16] we do not only have to add the stipulation that L has to become head of the target PE, but also that an L-head is only well-formed in a target PE that contains ʔ independently of N-prefixation. The language-specific restriction that (lexically generated) PEs containing an L-head must also contain ʔ is backed up by the fact that there are languages with as series of (non-neutral, prenasalised) voiced stops (containing an L-head and a ʔ-operator) but without a series of voiced fricatives (also containing an L-head but no ʔ).[17] The same restriction can also account for the non-voicing of postnasal w j h in Zoque without loss of the N-prefix. This follows if we assume that in Zoque, like in other languages, w j h contain an H-head but no ʔ, so we have w (U · H̲), j (I · H̲), h (H̲). For example, in French and Low German, we find that v can be a constituent governor (French vrɛ 'true', Low German vrɑk 'wreck'). In Standard (High) German or English, on the other hand, v is a bad constituent governor, because it does not contain H. Similarly, in English or High German, h/x may not be a constituent governor while it may be in Polish or Russian (Polish xlep

[16]The cognitive representation of l r is subject to some acoustic cue overlap. Which combination of ʔ and the aperture elements is the correct one for any given language, has to be established cognitively and cannot be derived from phonetic notions like 'liquid', 'lateral' or 'trill'.

[17]The application of the constraint on the targets of L-moving or -spreading operations, apparent in Zoque, that lexical L-heads must license ʔ might be an argument in favour of the proposal that such phenomena involving nasality are not phonological (*i.e.* derived) but lexical. For argument's sake, let us continue to assume that they are phonological. As will become clearer below, the point I am trying to make has more to do with the acquisition of phonology than with whether or not the phenomena under discussion are lexical or derived.

'bread'). Furthermore, in many languages exhibiting nasal harmony (spreading at the skeleton), w j may be nasalised, in some in may not be. Since such opacity effects find their main motivation in the aforementioned L/H-incompatibility and the presence of H in opaque PEs (*cf.* Ploch [376]), it can be said that the assumption of an H-head for Zoque w j h is still new and requires more research but is not completely *ad hoc*. It follows from this that, firstly, L (or ʔ parasitically on L) cannot move to stem-initial w j h because such targets would not comply to the constraint that target PEs of L-fusion must contain ʔ independently of N-prefixation, and secondly, that N (which is not able to move) can still be inter-onset licensed by w j h which contain an H-head.[18]

In the case of the affixation of a stop-initial suffix to a nasal-final stem, the directionalities of the inter-onset licensing and opposite inter-onset parsing relationship are reversed. In such an environment, there is no spreading of the aperture element into suffixes. Since all cases of homorganic nasals (preceding C) involve fused PEs, there is no evidence for any aperture-element spreading in Zoque. Note that this is identical to Korean nasality-induced voicing (*cf.* section 7.1; data in (11) from Wonderly [506, p. 120] and [507, p. 155]):[19]

(11) *Nasality-induced voicing in Zoque suffixation*

Stem	*Incompletive*	*Gloss*
nɨm-	nɨm-ba	'he says'
min-	min-ba	'he comes'
maŋ-	maŋ-ba	'he goes'
kaju-poks-	kaju-poks-pa	'he rides horseback'

Let me point out that I do not consider it problematic that, firstly, languages vary with respect to whether H contained within the target PE of a nasalisation phenomenon may be knocked out by spreading L and, if it may, whether only H-heads

[18] I neglect here why non-harmonised N is not realised [ŋ] but [n]. Note also that due to the move-or-get-lost constraint, there are no cases in Zoque where the aperture element spreads from stem-initial onset to N.

[19] 'ʌ' has been substituted with 'ɨ'. Regarding the incompletive suffix -pa, *cf.* Wonderly [507, p. 155]. I did not find examples containing -pa with vowel-final stems. One could try to say about the data in (11) that they do not argue for L-spreading because the voicelessness in suffix-initial p could be derived from underlying b in kaju-poks-pa via H-spreading from s (H · A) or (H · A · I). Since there is underlying b (neutral stop) in Zoque, one could wonder whether Wonderly is mistaken in his assumption that the suffix-initial stops in Zoque are underlyingly voiceless. However, in [507], there does not seem to appear a suffix-initial contrast between so-called voiced and voiceless stops, and in all cases where a stop-initial suffix follows a vowel-final stem, the stop is voiceless, *e.g.* -tɨh in ʧɨkhaja-t̠ɨh-u 'he had it done for him' [p. 156].

or all H-elements may be pushed out, and, secondly, that languages differ in relation to the precise nature of the link between the acoustic cues 'voiced and voiceless stops/obstruents' and cognitive PEs containing ʔ and L or H. I have already explained in [383] and chapter 4 in what way the phonology is motivated by language acquisition. More specifically, my proposal is that cases of acoustic cue overlap in vowels can only be disambiguated by the occurrence of certain underlying contrasts or processes. My analysis of nasality-induced voicing confirms the claim of the (neo-)RET that such overlap exists for consonants and shows for the cases discussed in this chapter that without L-spreading, -moving and/or -delinking, it would in many cases be impossible to decide whether, *e.g.*, a voiceless stop is 'neutral', *i.e.* neither contains L nor H, or contains H, whether a voiced stop is neutral or contains an L-head or whether a prenasalised stop is linked to two onset points or is fused and attached to only one onset point. Again we find support for my claim that the phonology provides information to the acquirer without which certain consonant systems could not be acquired.[20]

Note that Zoque ʔ patterns with other onsets which cannot inter-onset license N. Also if ʔ contained a ʔ-element, this would make ʔ eligible for L-fusion, which it is not. However, there is to my knowledge no language in which the PE realised [ʔ] can govern any other onset. The only case (that I have seen) where this might be suspected is the past participle prefix in Southern German (*cf.* Dahl's Law in Augsburg Swabian ('AS'), chapter 5). Interestingly, in AS the prefixal onset (which dominates k (ʔ · $\underline{\text{H}}$) at LL) can be inter-onset governed by stem-initial ʔ (kʔɛndərt^h in (16b.)). Nevertheless, this situation provides no argument in favour of the claim that the PE motivating ʔ is a good inter-onset licenser in Zoque because, as I have argued in chapter 5, in AS (or Kikuyu, for that matter), H-head dissimilation is the only constraint on the licensing relationship labelled 'Dahl's Law'; so an H-operator linked to the inter-onset licenser in AS does not trigger Dahl's Law because it cannot license the preceding onset. Similarly, in Zoque, only an H-head can license the preceding onset. So in both AS and Zoque, the PE motivating p patterns with PEs which do not contain an H-head. Taking into account such considerations and the opacity of ʔ to nasal fusion in Zoque, I conclude that ʔ must either be motivated by (H) or be empty.

[20]Note that there are languages which seem to lack any process that could possibly be interpreted as 'disambiguating' in this sense. Many Polynesian languages are of this type (*cf.* my article on Lio [377] (in preparation)). Such languages are interesting in relation to the study of acoustic cue overlap because they provide examples of systems which the acquirer can apparently learn without disambiguating processes. A system with the three underlying vowels i u a would, for example, not require a disambiguating process.

In both cases the absence of an H-head would prevent inter-onset licensing and, since L could not spread due to the missing ʔ-element in the lexical form of stem-initial ʔ, this would result in N being lost before ʔ. On the basis of these facts I cannot decide whether ʔ is the phonetic realisation of an empty onset or of (H).

Finally, let me point out that the reason that I do not propose for Zoque a lexical consonant system similar to AS, *i.e.* with 'voiced stops' exhibiting a fused H-head and 'voiceless' ones an unfused one, is that I presume in line with the (tacit) assumption within the (neo-)RET that the Korean (or the AS) system with two types of stops containing an H-head is, *de facto*, more marked than one with only fused H-heads. In other words, building on this tacit assumption and in agreement with my proposal of the Acquisitional Hypothesis (chapter 4), I assume that an acquirer will only assume unfused H-heads if necessary, *i.e.* if there is conclusive evidence for doing so. In Korean and AS, there is such conclusive evidence; in both languages, the attested restrictions on (leftward) inter-onset licensing can only be predicted if one assumes that only H-heads govern and that there are two types of H-headed PEs in the lexicon of these languages, fused and unfused ones. In Zoque, on the other hand, a system with 'voiceless' PEs containing a fused H-head and 'voiced' neutral PEs suffices to explain N-prefixation. Note that the proposal of more and less marked structures—be they melodic or prosodic—is not new. However, whereas phonetically grounded versions of markedness conventions contain an unfalsifiable version of the PH, a cognitive approach does not suffer from phoneticism. An empirical source for the establishment of default consonant systems are languages with few or no phonological processes or phonotactic constraints. Since in such languages the acquirer has to phonological help in disambiguating theoretical possible acoustic cue overlap, the systems exemplified by them must be unambiguous.[21]

To sum up, I propose the following underlying representations for Zoque consonants:[22]

[21] I investigate this view in some more detail in [377].

[22] Whether or not ʧ contains an A-element or whether s contains an A and/or I-element is not relevant here. Also, I neglect the precise nature of the distribution of ʔ and the aperture elements in l r.

(12) *Internal representations of lexical consonants in Zoque*

p	(ʔ · U · H̲)	ʔ	–/(H)
b	(ʔ · U)	f	(U · H)
t	(ʔ · A · H̲)	s	(A · H)
d	(ʔ · A)	ʃ	(A · I · H)
ʧ	(ʔ · A · I · H̲)	m	(ʔ · U · L)
k	(ʔ · H̲)	n	(ʔ · A · L)
g	(ʔ)	ɲ	(ʔ · I · L)
w	(U · H̲)	l	(ʔ · A)
j	(I · H̲)	r	(A)
h	(H̲)		

Having looked at Zoque prenasalisation, let us now study a similarly constrained example of N-prefixation: the Indonesian case.

7.4 Indonesian

Let me start this section by introducing the relevant data (taken from Sneddon [452, pp. 9ff.]; translations from Wojowasito [501]). In Indonesian, there are two prefixes ending in N: məN- and pəN-. N in these prefixes initiates a number of changes when affixed to a stem. To illustrate this, I will use examples containing məN-:[23]

(13) *The Indonesian* məN-*prefix*

a. ŋ before vowels

⟨ajar⟩	aʤar	⟨mengajar⟩	məŋaʤar	'teach'
⟨ekspor⟩	ekspor	⟨mengekspor⟩	məŋekspor	'export'
⟨erang⟩	ərang	⟨mengerang⟩	məŋəraŋ	'groan'
⟨iri⟩	iri	⟨mengiri⟩	məŋiri	'envy'
⟨olah⟩	olah	⟨mengolah⟩	məŋolah	'make fun'
⟨urus⟩	urus	⟨mengurus⟩	məŋurus	'attend to'

[23]Similar data can be found in any Indonesian grammar, *e.g.* Alieva *et al.* [2], Kähler [244]; Halle & Clements [197, p. 125] also provide an exercise with the relevant data. məN- usually derives transitive verbs. Also, stems beginning with ⟨f v sy z kh⟩ (f v ʃ z x) are usually only found in loanwords. Furthermore, foreign stems with initial consonant clusters (branching onsets in the source language) whose first consonant is a voiceless stop, *e.g.* ⟨proklamasi⟩ 'proclamation', do not lose the voiceless stop when the prefix ending in N is added: ⟨memproklamasikan⟩ məmpro... 'to proclaim, announce'. Sneddon [*ibid.*] provides no example with stem-initial i; I have taken ⟨mengiri⟩ and ⟨iri⟩ from Wojowasito [*ibid.*]. Note, Indonesian w sounds like [β] or [v].

b. N + p t k → m n ŋ

⟨pakai⟩	pakai	⟨memakai⟩	məmakai	'wear, put on'
⟨tulis⟩	tulis	⟨menulis⟩	mənulis	'write'
⟨kirim⟩	kirim	⟨mengirim⟩	məŋirim	'send'

c. homorganic N before b d g f ʧ ʤ ʃ z h x

⟨beli⟩	bəli	⟨membeli⟩	məmbəli	'buy'
⟨dengar⟩	dəŋar	⟨mendengar⟩	məndəŋar	'hear'
⟨ganggu⟩	gaŋgu	⟨mengganggu⟩	məŋgaŋgu	'annoy'
⟨fitnah⟩	fitnah	⟨memfitnah⟩	məmfitnah	'slander'
⟨cari⟩	ʧari	⟨mencari⟩	məɲʧari	'look for'
⟨jual⟩	ʤual	⟨menjual⟩	məɲʤual	'sell'
⟨syarat⟩	ʃarat	⟨mensyaratkan⟩	məɲʃaratkan	'set as a condition'
⟨ziarah⟩	ziarah	⟨menziarahi⟩	mənziarahi	'visit a grave'
⟨hilang⟩	hilaŋ	⟨menghilang⟩	məŋhilaŋ	'disappear'
⟨khawatir⟩	xawatir	⟨mengkhawatirkan⟩	məŋxawatirkan	'alarm, frighten'

d. N + s → ɲ

⟨sewa⟩	sewa	⟨menyewa⟩	məɲewa	'hire, rent'

e. N before w j m n ɲ ŋ l r is lost

⟨wakil⟩	wakil	⟨mewakili⟩	məwakili	'represent'
⟨yakin⟩	jakin	⟨meyakini⟩	məjakini	'consider certain'
⟨masak⟩	masak	⟨memasak⟩	məmasak	'cook'
⟨nanti⟩	nanti	⟨menanti⟩	mənanti	'wait (for)'
⟨nyanyi⟩	ɲaɲi	⟨menyanyi⟩	məɲaɲi	'sing'
⟨nganga⟩	ŋaŋa	⟨menganga⟩	məŋaŋa	'be agape'
⟨lihat⟩	lihat	⟨melihat⟩	məlihat	'look (at)'
⟨rasa⟩	rasa	⟨merasa⟩	mərasa	'feel'

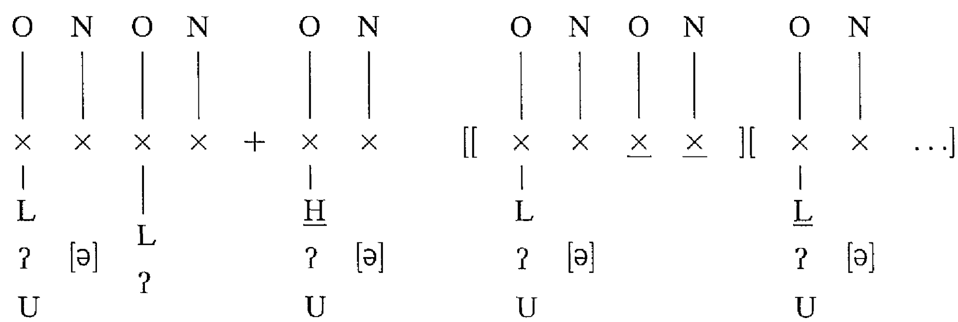
One of the differences between N-prefixation in Zoque and in Indonesian is that, while in Zoque both voiced and voiceless stops are affected in the same way (both come out as prenasalised voiced stops), in Indonesian, only voiced stops result in prenasalised voiced stops (bəli, məmbəli), voiceless stops are totally nasalised, resulting in *one* nasal consonant which exhibits the same aperture element that also the underlying voiceless stop contains (pakai, məmakai). We know that Indonesian voiced stops cannot be L-headed because the result of prefixing N to voiced stops already gives us L-headed PEs (prenasalised voiced stops). However, if as in Zoque, voiced stops were neutral, *i.e.* contained no L- or H-element, there would be no explanation for the fact that Zoque voiced and voiceless stops pattern together while the corresponding acoustic cues in Indonesian diverge in their phonological behaviour. Note that we cannot simply claim that in Zoque L moves from a source to which it is assigned as operator to a target to which it tries to fuse as head, while in Indonesian, L attempts to fuse with the target onset PE as operator. Assuming the same underlying consonantal system (at least with respect to PEs motivating voiced and voiceless stops) that we also assume for Zoque, we would predict that underlying b ($ʔ \cdot U$) turns into m ($ʔ \cdot U \cdot L$) and underlying p ($ʔ \cdot U \cdot \underline{H}$) into (acoustically identical or similar but phonologically different) p ($ʔ \cdot U \cdot L \cdot \underline{H}$)—if L and H may fuse in Indonesian—, or, if the H-head in lexical p can be knocked out, into mb ($ʔ \cdot U \cdot \underline{L}$). Since these predictions are not correct for Indonesian, we cannot explain N-prefixation as operator-operator move (as opposed to a operator-head move in Zoque).

Let me therefore propose that in Indonesian, as in AS, both voiced and voiceless stops are H-headed; 'voiced' stops contain a fused H-head, voiceless ones an unfused H-head. This way, Indonesian voiced stops are motivated by the same PEs that Zoque voiceless stops are cognitively motivated by, *i.e.* by PEs containing ʔ and a fused H-head (*e.g.* Zoque p, Indonesian b ($ʔ \cdot \underline{H}$)), while Zoque voiced stops are neutral (*e.g.* b ($ʔ \cdot U$)), and Indonesian voiceless stops contain ʔ and an unfused H-head (*e.g.* p ($ʔ \cdot U)(\underline{H}$)). N-prefixation can now be understood in the following way: As in Zoque, there is a head-final licensing relationship holding between stem-initial onset and the onset dominating N. L and H may not fuse within one and the same PE, and N from the prefix fuses with stem-initial b ($ʔ \cdot U \cdot \underline{H}$) by knocking out the H-head and becoming head of the resulting PE: mb ($ʔ \cdot U \cdot \underline{L}$). N fuses with underlying p ($ʔ \cdot U)(\underline{H}$)—which contains *unfused* $\underline{H}$—by becoming an operator; since L and H are incompatible and L can push out H, H is delinked. This results in m ($ʔ \cdot U \cdot L$).

The question which arises here is: what is the reason that L does not fuse with ($ʔ \cdot U)(\underline{H}$) in a way such that it knocks out H and becomes, as in the case of Indonesian voiced stops, head of the resulting expression (mb ($ʔ \cdot U \cdot \underline{L}$))? Let me propose that this has to do with the observation that in Indonesian, L may only become head of the target PE if the target PE is lexically headed. More specifically, underlying b (ʔ·U·H̲) consists of one PE, which contains one fused head; L knocks out this head and substitutes it. However, in the case of p (ʔ · U)(H̲), we have one PE containing an unfused head. L cannot knock out this unfused H-head because, universally, L cannot become an unfused head. We do not why this is so, but there is no evidence independent of Indonesian which would suggest the existence of unfused L-heads. Since (ʔ · U)(H̲) does not contain a fused head and since L can only become the fused head of the target PE if that PE is lexically licensed to have a fused head, L can only knock out H of a Indonesian voiceless stop by fusing with the target PE as operator, resulting in m (ʔ·U·L). As we have seen above in Zoque, L fusing with the stem-initial target PE has as one of its consequence an empty syllable. Since we have fusion with both voiced and voiceless stops in Indonesian, we get an empty syllable in both cases:[24]

(14) *Indonesian* məmbəli 'buy' (*from* bəli)

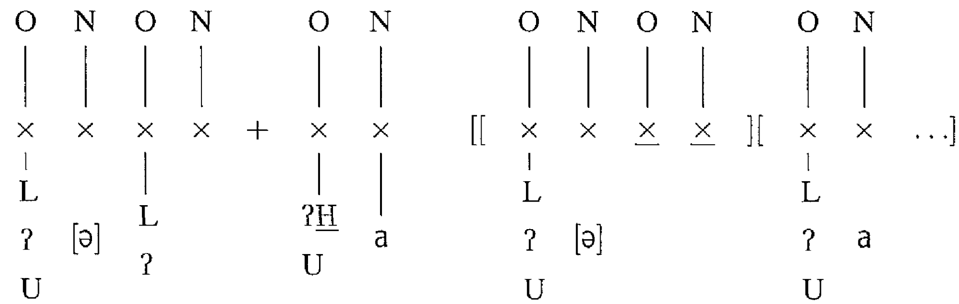
a. məN- + bəli *at LL* b. məmbəli *at PL*



[24]I assume Indonesian ə to be the phonetic realisation of an unlicensed empty nucleus. Also, since mp is well formed in Indonesian domains where no məN-prefix is involved and since the result of the prefixation of məN- sometimes contains a parsing cue, *e.g.* ŋh—with this sequence being ill-formed within one domain—, I assume this prefix to be analytical. However, it appears that məN- when preceding the prefix ⟨per-⟩ pər-, resulting in ⟨memper-⟩ məmpər- (*məmər-), forms its own lexical entry as one and the same (non-analytical) domain [məmpər-]. Also note that in the following constituent structures of prefixed Indonesian forms, only the first ON-pair of the stem (counting from the left) is given.

(15) *Indonesian* məmakai 'wear' (*from* pakai)

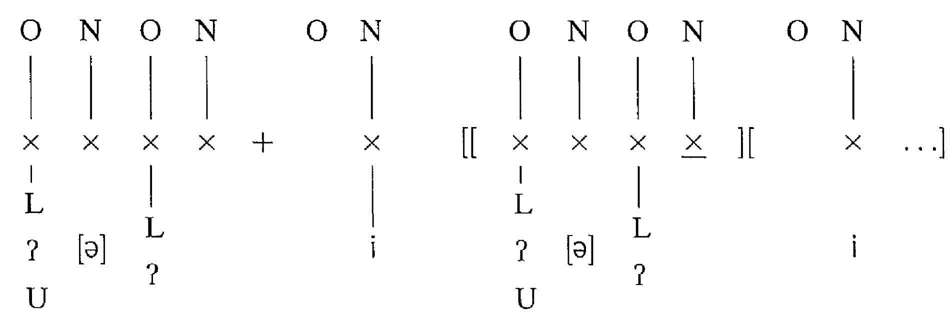
a. məN- + pakai *at LL* b. məmakai *at PL*



I assume vowel-initial stems to start with an empty onset (which does not dominate a point, *cf.* (11a.) in chapter 6, p. 238). Since only points can be licensers in GP, this results in məN- simply being prefixed without N trying to fuse with the stem-initial onset. One could ask why N is well-formed then at all and is not delinked as in the case of stem-initial glides (*cf.* below). My answer to this is that N in Indonesian is subject to the same licensing condition that also N in QF or MF is subject to (*cf.* chapter 6): N does not have to be inter-onset licensed in order to be licensed to be attached to its onset point; this explains why in QF domain-final NVs and in MF domain-final Vŋ-sequences are well-formed. For both QF and MF, and in addition, for Indonesian N we can say that, if the onset dominating N is followed by another onset point to its right—which happens in Indonesian after concatenation of the məN-prefix—that following onset *must* inter-onset license N. Therefore bad licensers, *i.e.* glides and nasals, may not follow QF NVs, MF VN-sequences or Indonesian N. The relevant prosodic representations are:

(16) *Indonesian* məŋiri 'envy' (*from* iri)

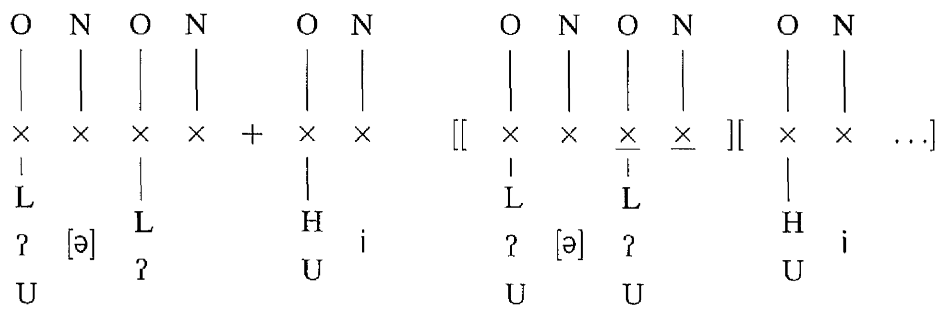
a. məN- + iri *at LL* b. məŋiri *at PL*



To be clear, I propose in line with the (neo-)RET that all PEs containing H (head or operator) or an L-head are well-formed governors of N. It follows from this that voiced and voiceless stops (which in Indonesian both contain an H-head), voiceless fricatives (with H-operator) or voiced fricatives (with L-head) but not glides or nasals are well-formed inter-onset licensers. Therefore, N preceding the latter group is delinked (resulting in an empty syllable) while N preceding fricatives remains. As in Zoque, if N does not fuse with the stem-initial PE, the aperture element(s) of the licenser move leftward to the onset dominating N. Note that for this to work, we have to assume that, opposed to Zoque, w j do not contain H in Indonesian, and ʧ ʤ do not contain an H-head. Also, as proposed, all PEs containing H are well-formed inter-onset licensers in Indonesian; in Zoque only PEs containing an H-head are grammatical in this function. Furthermore, as in Zoque, we need to assume the lexical presence of ʔ in the target PE of an L-fusion operation. Thus Indonesian f (containing an H-operator) can inter-onset license N (expressed via spreading of the aperture element)—even though it cannot in Zoque) but can, because it does not contain ʔ, not take part in L-fusion. We get:

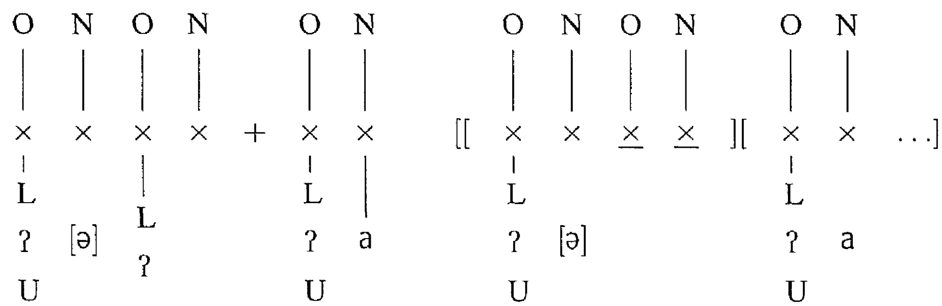
(17) *Indonesian* məmfitnah 'slander' (*from* fitnah)

a. məN- + fitnah *at LL* b. məmfitnah *at PL*



(18) *Indonesian* məmasak 'cook' (*from* masak)

a. məN- + masak *at LL* b. məmasak *at PL*



Furthermore, the phonological behaviour of s shows that, in Indonesian, s, firstly contains an I-element, resulting in a so-called 'palatal' nasal consonant in N-prefixation contexts, and secondly, contains an ɦ-head. Whether Indonesian s contains an A-element I cannot solve here. Since s can be nasalised, it also must exhibit ʔ in its internal representation. In other words, s behaves like a 'palatal stop' in Indonesian (ʔ · I · H̲), while ʧ patterns with PEs containing an H-operator, *i.e.* with voiceless fricatives. Note that it is the phonological behaviour of s, not its phonetic properties which tell us something about its internal make-up. Indonesian z patterning with ʤ simply contain an L-head (but not glottal element). Both properties prevent L and thus ʔ from fusing with z ʤ; the L-head licenses N staying attached to its lexically assigned onset. h x behave like f; I assume them to contain an H-operator, licensing N to remain linked. The absence of a ʔ-element as part of their internal representation prevents N from fusing with them.

To sum up, I have shown in this section that Indonesian voiced and voiceless stops are motivated by the same PEs that AS stops are motivated by. Neglecting some of the details, it can be said that both voiced and voiceless stops contain an H-head in Indonesian; voiced stops have a fused H-head, voiceless stops an unfused one. Voiced fricatives, on the other hand, contain an L-head. The most important findings of this section is that phonetically defined cues like 'voiced' or 'voiceless' for PEs do not contain much useful information in relation to the phonological behaviour of the PEs involved. It always has to be decided independently of the phonetics which phonetic detail is relevant and what it is motivated by.

Conclusion

In this chapter, I have provided evidence in favour of my claim that the phonology is not motivated phonetically and that a cognitive account is the more empirical one by pointing to cases of acoustic cue overlap apparent in the way how the consonant systems of different languages (Korean, Ecuador Quichua) deal with nasality-induced voicing. Also, I have illustrated with such cross-linguistic evidence that many consonant systems could not be acquired without the helping, *i.e.* disambiguating, hand of phonology.

Conclusion

In this thesis, I have compared two approaches to the phonology of nasality: the phonetic (part 1) and the cognitive approach (part 2). In part 1, I have demonstrated that the mainstream assumption that the phonology is motivated by the phonetics is flawed. It has become clear that phonological nasality cannot be predicted on the basis of phonetic measurements. Notions commonly referred to as phonetic motivations for nasality, *e.g.* velum opening or lowering or nasal airflow, cannot predict the behaviour of any phonological units. Neither systems of contrast nor phonological processes can be explained in phonetic terms; they may merely be describable in such terms. Furthermore, there is no well-defined phonetic framework which would back-up notions like phonetically grounded 'nasality'. In addition, I have provided evidence which shows that the PH is always set up in an unfalsifiable or nearly unfalsifiable and therefore mythological or nearly mythological manner. Feature theories which employ the feature [nasal] are thus not only metatheoretically flawed but also overgenerate or must insist on not-applying the PH when they would otherwise be proven wrong. Unfortunately, the PH never adds anything to a phonological explanation of a linguistic phenomenon. This can also be said in relation to the behaviour of nasality in the historical development of languages. I have argued that, contrary to the views held by a number of typologists, the widely accepted Heightmyth is not supported by evidence. That is to say that the assumption that there is a phonetically motivated phonologically relevant connection between nasality and vocalic height or consonantal place of articulation is not an empirical one.

In the second part of this thesis, I have outlined a cognitive approach to the phonology of nasality. After an introduction to Government Phonology and its subdisciplines Element Theory and the Theory of Generative Constraints, I have provided evidence via an analysis of the nasal vowels in Jula, Mahu and Lobiri that phonologically nasal vowels may be phonetically oral. This argues in favour of a cognitive approach and against a phonetic one. Furthermore, I have shown that there is numerous cross-linguistic evidence for merging KLV's N- and L-element into one new element, (new) L. This merger helps to avoid overgeneration of PEs predicted to occur world-wide and is necessary to establish a unified account of Dahl's and Meinhof's Law.

Importantly, I have demonstrated that, as in the case of the vowel systems on the basis of which vowel harmony operates in a number of Turkic languages, the consonants systems of many languages cannot be acquired without the help of certain phonological processes disambiguating the apparent acoustic cue overlap. Phonology is an acquisition tool made necessary by the underdetermined nature of the acoustic cues employed by human languages. This means that it is not surprising at all that the phonetically motivated approaches discussed in part 1 of this thesis are as unsuccessful and non-empirical as described: It is not only the case, as current GP assumes, that the phonetic signal contains all of the relevant phonology but that there is so much phonetic packaging that an acquirer/listener has to know (as part of their genetic endowment) which small details of the acoustic string he/she has to look for in order to decipher the message. My research shows that, in addition, the phonetic signal does not even contain all of the relevant information. I claim that human languages employ vowel and consonant systems, *i.e.* lexical systems of oppositions, (static) phonotactic constraints and (dynamic) phonological processes in order to provide the acquirer with information about the system to be acquired. Without such information, acquisition would fail. It can therefore be said in relation to my study of the (new) L-element that voicing and aspiration contrasts for consonants and phonotactic constraints on such oppositions are not phonetically motivated but provide the acquirer of a language with the information he/she needs in order to decide on the internal (cognitive) representations of consonants.

In short, the main contributions this thesis makes to the study of phonology are a detailed deconstruction of the PH using the example of nasality, its rejection of the PH (whose sell-by date is long past), and the proposal that the acoustic code employed by natural language contains phonologically irrelevant information (*i.e.* is overdetermined) and, at the same time, does not contain all of the information needed to acquire the cognitive units involved (*i.e.* is underdetermined). Other than a parsing tool and a lexical addressing system, phonology is mainly an acquisition device.

Abbreviations

∅	empty nucleus
∅	not applicable
single arrows (*e.g.* →)	change by a synchronic process
double arrows (*e.g.* →)	change by a diachronic process
⇔	is etymologically related to
⇎	is not etymologically related to
[ ]	1. bibliographical references, 2. comments by myself (within quotes), 3. phonological domains
( )	1. references to other parts of this volume, 2. comments by myself (outside quotes)
*	precedes ill-formed material
°	precedes reconstructed forms
†	precedes non-assonating forms
+	yes, exists
–	no, does not exist
$\overset{o}{\sim}$	1. optional (as opposed to + 'yes' and – 'no', 2. ($\overset{o}{\sim}$)x $\overset{o}{\sim}$y: x is in optional variation with y

adj.	adjective
AP	Archangeli & Pulleyblank
BB	Bermeo Basque
BKG	Beddor, Krakow & Goldstein
BL	Benguerel & Lafargue
BP	Brazilian Portuguese
BSF	Brun, Spencer & Fourcin
CG	Charette & Göksel
DH	Dolbey & Hansson
ECP	Empty Category Principle
ESC	Empty Syllable Constraint
f.	feminine
GRS	group recognition system
HA	head alignment
HM	Height Myth
KLV	Kaye, Lowenstamm & Vergnaud
LAD	Language Acquisition Device
m.	masculine
MF	Montpelier French
NC	nasal consonant
NV	nasal vowel
OC	oral consonant
OCP	Obligatory Contour Principle
OV	oral vowel
PNC	primary nasal consonant
QF	Québec French
sg.	singular
SG	Standard German
VHP	Vowel Height Parameter
VLP	Vowel Length parameter

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Bibliographical abbreviations

AUA	*Annales de l'Université d'Abidjan*
BLS x	*Bulletin of the x-th Annual Meeting of the Berkeley Linguistics Society*
BSOAS	*Bulletin of the School of Oriental and African Studies*
CJL	*Canadian Journal of Linguistics*
CLS x	*Papers from the x-th Regional Meeting of the Chicago Linguistic Society*
IJAL	*International Journal of American Linguistics*
JALg	*Journal of African Languages*
JASA	*Journal of the Acoustic Society of America*
JCL	*Journal of Chinese Linguistics*
JL	*Journal of Linguistics*
JPhet	*Journal of Phonetics*
JSHD	*Journal of Speech and Hearing Disorders*
JSHR	*Journal of Speech and Hearing Research*
L	*Linguistics*
LA	*Linguistic Analysis*
Lg	*Language*
LI	*Linguistic Inquiry*
MIT WP-L	*MIT Working Papers in Linguistics*
NELS x	*Papers from the x-th Annual Meeting of the North Eastern Linguistic Society*
NLLT	*Natural Language and Linguistic Theory*
SAL	*Studies in African Linguistics*
SELAF	Société d'Etudes Linguistiques et Anthropologiques de France
SOAS WP-LPhet x	*SOAS Working Papers in Linguistics and Phonetics. Volume x*
UCL WP-L	*UCL Working Papers in Linguistics*
UCLA WP-Phet	*UCLA Working Papers in Phonetics*

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